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THE NEW PHILOSOPHY OF ANALYSIS

BRAND BLANSHARD

Professor of Philosophy, Yale University

(*Read November 8, 1951*)

A LITTLE more than twenty years ago I was asked by the *Journal of Philosophy* to report for it the International Congress of Philosophy at Oxford. One of the papers at the Congress was read by the professor of philosophy at Vienna, Moritz Schlick. He took the position that the sole business of philosophy is to analyze and clarify what is said by science. I do not think that most of his hearers took him very seriously; I was so far from doing so myself that I allowed myself in the report to be somewhat facetious about what seemed an absurd statute of self-limitation for philosophy. This paper may be taken as partial penance for that attitude. For Schlick's venture has become a movement, and what is more, perhaps the most influential movement of the century in the philosophic field. It has spread from Austria to England, where it has, in somewhat different forms, made a conquest of the three chief universities, Oxford, Cambridge, and London; it seems to have achieved a position of dominance in the Scandinavian countries; and it has moved on to the United States, where some of its chief representatives came as emigrés, and where it has installed itself in positions of influence at Harvard, Cornell, Chicago, Minnesota, and the University of California. This school of logical positivism or logical empiricism is not the only form of the new philosophy of analysis, but it is the most developed, articulate, definite, and influential. What I wish to do in the few minutes I have with you is first to state the three main theses that it stands for, and then to show some of the difficulties it has encountered in establishing the first of these.

All three of the theses of logical empiricism are of the greatest importance for philosophy. Indeed if they were accepted, philosophy in its traditional sense would virtually cease to exist. It is worth remarking that all the members of the *Wiener Kreis,* the original Vienna circle, were in the first instance scientists, and that they were reacting, under the influence of their distinguished teacher, the physicist Ernst Mach, against not only speculative philosophy, but also the speculative elements in natural science.

The first thesis was a proposal for determining whether a statement about fact was meaningful or not. The positivists, returning for guidance on this point to Hume and Mach, said that the meaning of a proposition was its mode of verification, and since verification meant verification in sense experience, statements not so verifiable could be ruled out as meaningless. Since a vast mass of the traditional statements of the philosophers did not pretend to be so verifiable—statements, for example, about the external world when it is not observed, about the past, about the infinite and the absolute, about God, about first and last things—these statements must all be thrown out as senseless. Regarding this, more in a moment.

The second thesis of positivism is almost equally important; it has to do with *a priori* knowledge. *A priori* knowledge, as Kant defined it, is knowledge that is necessary and universal; any proposition of logic or arithmetic will provide an example. Philosophers, as you know, have been puzzled ever since the days of Plato as to how we could have such knowledge; how is it that we can know beforehand that if we go to the remotest Pleiades, we shall find that two and two still make four? Kant thought the only way to account for it was to say that all the necessities we find in nature are forms of order imposed by ourselves; but that landed him in skepticism about the world that fell beyond our experience. The rationalists from Descartes on held that such knowledge gave real insight into the structure of things. John Stuart Mill and the traditional empiricists denied that these propositions were necessary at all; if two lines could not enclose a space, that was because immemorial experience had forced it on us with never an exception, and we had now lost the power of conceiving an exception. The positivist view of *a priori* knowledge is that the rationalists and Mill were both right and both wrong. The ration-

alists were right against Mill in holding that such propositions were really necessary and exceptionless; but Mill was right against the rationalists in holding that we have no necessary knowledge of nature. For an *a priori* statement, says the positivist, is only an explanation of our own meaning, or—what is the same thing—a statement of how we propose to use words. A straight line is the shortest line; yes, naturally, because in a Euclidian context we should refuse the name of straight to any line that was not the shortest; that is part of what we mean by straight. Two and two are four; why of course, four is just another name for what we have already described as two and two. So of all *a priori* or necessary statements. They are certain merely because they set out explicitly what we already meant; they are explications of our meaning. That is what the positivists are contending when they say that the whole of logic and mathematics consists of tautologies. All necessary statements and inferences are analytic of meanings already present; hence they cannot give us new knowledge or tell us anything about the world. Why has this contention aroused so much excitement and opposition among philosophers? Plainly because if it is made out, it cuts the umbilical cord of speculative philosophy. The chief instrument of that philosophy, logical reasoning, is, by itself, worthless as a means of gaining knowledge about the nature of things.

The third contention of the positivists is an inference from the other two. If the only meaningful statements we ever make fall into one or other of these two types, if they are either statements of fact which can be verified in sense or analytic statements of what we mean, what are we to do with judgments of value? When one says that pain is intrinsically bad and pleasure good, or that an act is right or wrong, one seems to be saying something that cannot be tested by any appeal to the senses and yet reports something about fact, about the experience or act in question. Positivist writers have met this challenge by another extremely bold stroke. They have denied that ethical and aesthetic statements are judgments at all. These assert nothing; they are neither true nor false. They are really interjections or exclamations, expressing the speaker's feeling about what he is thinking of. The fullest exposition of this view is given in a recent work by a young American empiricist, Charles Stevenson of the University of Michigan, but perhaps its most influential support has come from a man who, though not quite a positivist himself, is one of the initiators of the movement, Bertrand Russell. The strength of the position lies in the fact that our feelings are obviously bound up with our value judgments in a way they are not with judgments of fact, a way never as yet very convincingly explained. The weakness of the position lies in the anarchy that it implies in the moral life. If it is true, it implies that the differences between east and west, for example, admit of no rational solution, that since nothing is asserted, no evidence can be adduced on either side, that in an issue like that of Korea there is no objective right or wrong in the case at all in accordance with which an international government could act. Russell admits that he is unhappy about his ethical theory, but says sorrowfully that he is unable to find a better.

We now have before us the three main positions of logical positivism. Perhaps I may say at once that I do not think that any of them has been satisfactorily made out, but since that is far too large a statement to make good in a few minutes, may I ask you to consider with me only the first of them, the positivist proposal for finding out whether a statement of fact is meaningful or not. To some persons the search for such a test seems an idle one. There is at times great difficulty, they say, in knowing what other people mean, but surely one knows, or with a little pains can know, what one means oneself. Unfortunately that is not true. It is a fact that has made trouble for philosophers ever since Socrates that one may be able to apply such words as "true," false," "good," "just," "thing," "cause," with confidence and apparent accuracy while yet being utterly unable to give a coherent account of what one means by any of them. Because of this difficulty, the positivists say, traditional philosophers have deceived not only their public but also themselves about the significance of what they were saying. On both sides of every important philosophical issue eminent speculators can be quoted who seem to be talking subtly, gravely, and profoundly, and the common man feels it hopeless to decide which of them is right.

The positivist here offers a suggestion with which it is hard not to sympathize, that perhaps both sides are talking nonsense. His proposal has the thoroughly commendable design of forcing philosophers to be clear, and of getting rid once for all of the obscurity that palms itself off as profundity. If we want to know whether a statement we make means anything, and if so what, we

must ask, he says, what sort of experience would assure us of its truth, what difference in our experience we should point to as making it true if we were challenged. What I mean when I say that there is an apple on the table in the next room is that if I should go and look, I should see one there. What I mean when I say that someone is untrustworthy is that if I give him a letter to post, or a secret to keep, or an office to run, I shall find that he does not behave as trustworthy people do. Generalizing from such cases, the positivists say that what we mean by any statement is the mode of its verification. And they add that when we ask how we verify any statement of fact, we find that it is always by reference to some sense experience—seeing how the apple looks or how the "untrustworthy" man behaves. If our statement names something such as the substance of the apple as opposed to any or all of its qualities, or an abstract character of untrustworthiness which we could never meet with in sense, then we know that we have been talking meaninglessly.

At first this seems clear enough. But on reflection questions arise. Does a statement always refer to the sense experience by which I should verify it? The most influential of the positivists, Wittgenstein, said Yes, since the experience by which anyone else might verify the statement is inaccessible to me. Very well, the reference of any statement is to the sense experience by which I, as distinct from anyone else, would assure myself of its truth. Unfortunately, this leads at once to shocking consequences. Suppose I make a reference to your thought of this moment, or to the toothache from which you may be suffering. Clearly, my only way of verifying this statement is through your behavior—your expression of interest, or your grimaces of pain, or your audible remarks. Since these are the experiences by which I should verify the statement, they must be what the statement means. In talking about your thought and feelings, then, I must always be talking about my own experiences of your bodily behavior; if I talk about anything else, such as your own private feelings, I am referring to something of which any sense experience on my part is impossible, and hence what I said would be meaningless. This was the conclusion soberly accepted by Wittgenstein, who described his theory as methodological solipsism.

Most philosophers have felt, and I think rightly, that this view is simply absurd. Granting that my only way of verifying your feeling of toothache is through your behavior, I surely do not mean by your ache the behavior through which you express it, still less my experience of that behavior. The positivist theory of meaning in its first interpretation leads not only to solipsism but to behaviorism; and behaviorism, as Professor Broad has said, is a silly philosophy; it declines to recognize the difference between the two things which of all things in the world are most clearly different, a sensation of pain, for example, on the one hand, and physical changes of nerve or muscle on the other.

Most positivists therefore abandoned this first position. They changed their thesis to read that though strong, that is conclusive, verification is still necessary, a statement is meaningful if it can be verified by anyone at all. You can speak of the mountains on the other side of the moon because, though you will never see them, this is owing to a lack of facilities which may yet be supplied, and you mean what would then appear. But if you speak of what can never present itself in sense to anyone, such as the Absolute, or the soul, or a disembodied spirit, or the good, you are still speaking nonsense.

This is certainly a great improvement on the earlier formulation. Nevertheless, it will not do. For one thing, it distorts the intention of all judgments of the past. When I say that Luther nailed his theses to a church door, I am clearly referring to an event which, since it is past, can never be verified in observation by anyone. Professor Ayer, indeed, proposed at one time to interpret the judgment of the past as meaning those future experiences by which we should try to verify it, such as the sentences we should find in a history of the Reformation. This seems to me a counsel of despair. And unfortunately the theory would set down as meaningless much else that we cannot dispense with. All realist philosophers believe, like plain men, that the tables and chairs of this room continue to be when unobserved, but since to observe the unobserved is in principle impossible, the theory before us would declare the suggestion meaningless, which it surely is not. Again, there is a host of entities in modern physics that apparently can never be observed for physical reasons, protons, electrons, photons, and so on; the positivists have been compelled to say that all we mean by these things is the pointer-readings or cloud-chamber paths which we take as evidence for them. Most physicists, if I am not mistaken,

would regard this as an illegitimate assimilation of the cause to the effect.

Such considerations have induced many positivists to amend their position again, and to substitute for strong verification the notion of weak verification. A statement is now meaningful, not if it can be conclusively verified in sense, but if it can be rendered in some degree probable by sense observation, if it involves any consequences in the way of sensible fact, if any empirical evidence can be given for or against it. Unhappily this new version is as much too wide as the earlier ones were too narrow. For all the propositions of metaphysics which the positivists sought to exclude now come trooping back. One can discuss the existence and character of God because the occurrence anywhere of pleasure or pain is relevant to it; one can discuss the past, and other minds, and the external world, and protons and electrons, because, though none of them may be verifiable directly, they are all believed by those who accept them to have effects in sense, which may therefore be used as evidence for them. The criterion of meaningfulness that began by excluding speculative philosophy has been forced by successive amendments to readmit such philosophy at the end.

I think this is typical of what is happening elsewhere in the movement. The basic trouble with logical empiricism is that it has not been empirical enough. Instead of forming its criterion of meaning from a study of what in fact we find meaningful, it has sought to impose a criterion in the interest of a preconceived theory as to what ought to be meaningful. Moral and speculative philosophy are not being destroyed by the analytic movement of the day. To be sure, they are having to examine their instruments with unprecedented care. But when those instruments are freed from the excessive rust that had accumulated on them, we may expect better and sharper service than they ever gave before.

EXCAVATIONS IN HOTU CAVE, IRAN, 1951, A PRELIMINARY REPORT

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Curator of Ethnology and Professor of Anthropology,
University Museum, University of Pennsylvania

(*Read November 8, 1951*)

1. CHRONOLOGY

THE cliff in which Hotu Cave lay concealed for several millennia first aroused our interest on January 8, 1949, on a motor trip along the Caspian shore road. We returned in October of the same year, and during the eight days from October 13 through October 20 we excavated Belt Cave, a grotto seen on the earlier trip. The report on this excavation has been already published,[1] but a few words of summary may be of use to those unfamiliar with its contents.

Belt Cave contained four cultural horizons, reading from top to bottom: (1) a mixed deposit containing Neolithic remains along with Iron Age, Islamic, and Modern materials; (2) a true Neolithic horizon divided into an upper (*2a*), which contained pottery and domestic animals, and a lower (*2b*), which contained domestic sheep and goats but no pottery; (3) a Mesolithic culture in which the principal food was supplied by a grassland or desert animal, *Gazella subgutturosa jacovlew,* still found on the Turkoman plain; (4) an earlier Mesolithic during which time the cave was a flint factory, and the workmen lunched off a small species of Caspian seal and many water birds. Under this was sterile clay, and under the clay the limestone bottom of the cave.

On October 21, after the trench at Belt Cave was finished, the workmen, wishing to ensure our return, told of a much larger cave, Hotu, the narrow mouth of which had been completely buried by the activities of dynamiters some fifteen years previously. Over the scene grass had grown, and no indication remained that such a cave had existed, other than a crack in the front of the roof, formed also by dynamite. With their help I was lowered into the body of the cave, and found myself knee-deep in bat dung. Since this cave was higher than Belt, it seemed likely that it might contain deeper and thus older deposits, and it was determined to dig it next season.

FIG. 1. Hotu and Belt Caves seen from road.

In February 1951 Mrs. Coon and I returned to Behshahr, followed shortly by Mr. and Mrs. Dupree, and set to work finishing the trench in Belt Cave, carrying it out to the open, and particularly trying to trace the bottom forward, to see how the deposit could have been made. The presence of the levels previously noted was confirmed, in particular the pre-ceramic Neolithic. Also the remains of a young family of four individuals were found buried at the threshold of the ceramic and pre-ceramic Neolithic, with a beautiful bone fork under the skeleton of the oldest female.

[1] Coon, C. S., Cave Explorations in Iran, 1949, *Univ. Museum Monographs,* Philadelphia, 1951.

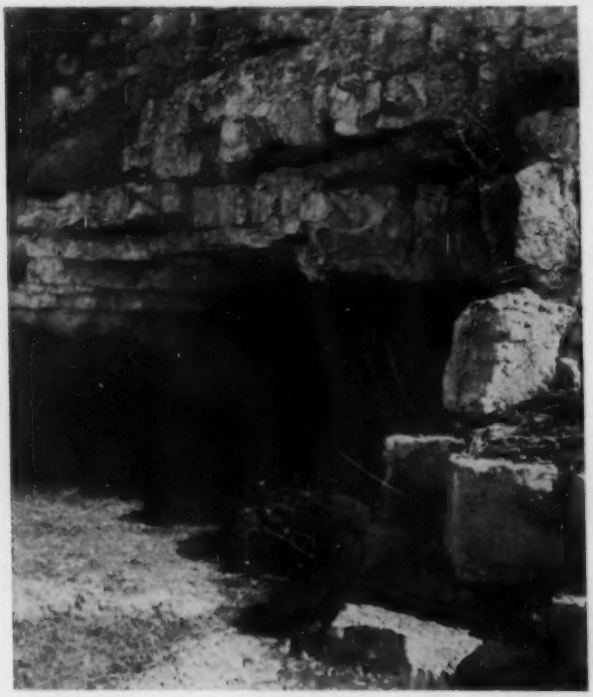

FIG. 2. Hotu Cave upper opening (1949) caused by blasting.

This work took from February 28 to March 13. Meanwhile, on March 2, a gang set to work opening the mouth of Hotu. This took until March 13, by which time the front of Belt Cave was excavated down to the sterile clay. Then we moved over to Hotu, leaving the completion of the Belt excavation to a smaller group. Between March 14 and April 21 the excavation of Hotu was carried on. First Trench A, a 5 by 3 meter square near the back of the cave, was dug to 7 meters until gravel was struck.

Since by this time the depth of the trench was over twice its width or length, and since the cave deposit was wet and shaky, we stopped out of consideration of the lives of our workmen. Then a contiguous section seven meters in length, called Trench B, was laid out. This, also, was carried down to the gravel. Realizing that the working space was still dangerous, we started to dig a third section, Trench C, outside the mouth, to let in light and provide an avenue of escape should the face fall. Fall, unfortunately, it did, during the lunch-hour on April 12, temporarily trapping three workmen in the rear of the cave. Luckily no one was hurt. Several days' work was needed to clear the debris. There was no question of continuing Trench C; time was growing short, the workmen were tired, and they were needed to plant tobacco. Very fortunately the soil that fell in from the south side of the cave and thus escaped stratigraphic study was nearly sterile; the cave dwellers preferred the north, or better-lighted, side of the cave.

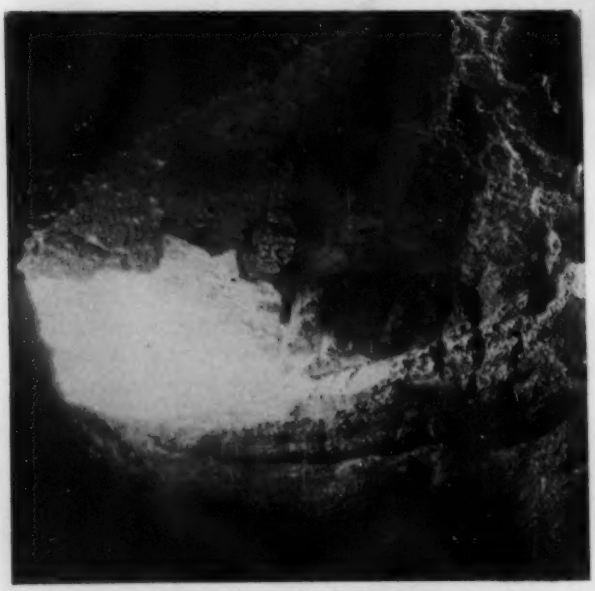

FIG. 3. Same opening from inside.

FIG. 4. Interior of cave showing original entrance filled to roof with Iron Age debris.

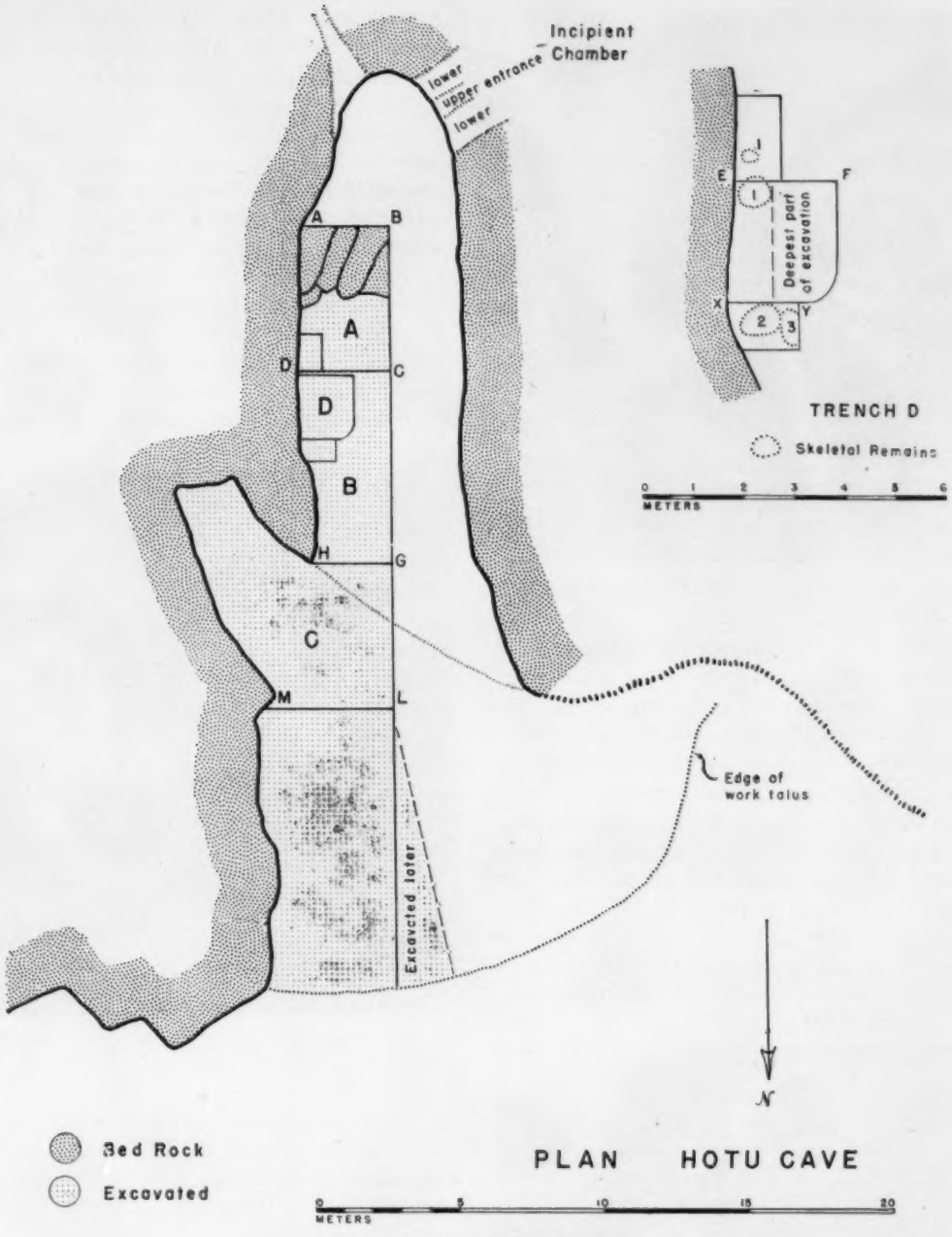

FIG. 5. Plan of Hotu Cave.

On April 15 we were ready to excavate the small part of the Pleistocene gravel surface previously exposed in Trenches A and B. It seemed reasonably safe as long as no one kicked anything down from above. Hence the small trench called D was made to the bottom of the Pleistocene gravel formed by a deposit of soft, wet kaolin, at 12.40 m. from the surface. At this point the walls of the cave were sloping in, and the bottom seemed near. On the reasonable grounds that it was too dangerous, the workmen refused to dig deeper, and there was too little time or energy left to broaden its base by clearing the whole cave to proceed downward. This task must be left to another season, at which time the mystery of what, if anything, lies under the kaolin may be solved. Work was complicated by

FIG. 6. Entrance as revealed by 1st opening.

the discovery in the fourth gravels of three skeletons along the edges of Trench D so situated that supplementary trenches had to be dug to get them out, thus narrowing the free area above to below the margin of safety. On April 20 the last two skeletons came out, and the dig was over.

Also on April 15, Mr. Robert Stevens, a professional photographer, arrived in time to make a complete photographic record of the last and most popular part of the excavation, the discovery and removal of the three geologically ancient skeletons. It is safe to say that no excavation of early man has been as well documented photographically. Hence the position of the bones, the stratification of the layers immediately above them, and the possibility of disturbance, can all be clarified by Mr. Stevens's pictures.

Back in Behshahr, aided by our hotel proprietor and the local pharmacist, both of whom made a midnight trip to Tehran to get acetone, we reconstructed the skulls as best we could with available materials, packed them, and loaded our vehicles for Tehran. There the finds aroused unexpected interest, including that of His Imperial Majesty the Shah.

Since our return to America, Dr. Lawrence Angel, assisted by Albert Jehle and Jane Goodale, has worked on the restoration of the skeletal material, concentrating on skeleton No. 2, the skull of which has since gone back to Iran. Mr. Dupree, who studied the pottery both in Tehran and Philadelphia, has for the purposes of this meeting concentrated on the Pleistocene flints, which he carried to Cambridge to show to Dr. H. L. Movius, Jr., and some of which Dr. Kenneth Oakley of the British Museum has also studied. The mammal bones have been identified partly by Dr. F. C. Fraser of the British Museum, partly by Mr. Fred Ulmer of the Philadelphia Zoological Garden and the Academy of Natural Sciences, and partly by my own preliminary efforts on the basis of Belt Cave bones identified last year by Dr. Henry Setzer of the Smithsonian Institution. Dr. Alexander Wet-

FIG. 8. Fissure in cliff to left of cave mouth. This was chute through which soil entered cave from above.

FIG. 7. Entrance opened to one meter level.

more has surveyed the bird bones prior to detailed identification. Dr. John Miller of Pennsylvania State College has studied the Pleistocene soil samples, and Dr. Frederick Matson of the same institution has begun a technical examination of the sherds from various levels. Dr. Donald McCown of the Oriental Institute of the University of Chicago has further examined the sherds for shape, decoration, and relationships to wares from other areas. Dr. William Libby of the University of Chicago and Miss Elizabeth Ralph of our own staff have begun the Carbon 14 analysis, giving us one tentative date in time for this paper. Dr. R. Olsen of the Museum of Comparative Zoology at Harvard has analyzed four animal and two human specimens from the Pleistocene gravels for fluorine content.

FIG. 9. Sifting and working platforms outside of cave.

These different pieces of evidence yield no clear pattern of agreement between them. I feel even less confident of my knowledge than I did on April 17 when I first saw Skeletons 2 and 3, and am possibly more confused.

2. GEOLOGY

The Elburz mountains, which look from the north like an unbroken wall, are actually divided by deep valleys into a number of minor ranges. One of these is the Kuh-i-Ja Khan Mura, which faces Asterabad Bay across a well-watered plain some two to four miles wide. Behind it, to the south, flows the Nika River, draining the fertile and populous valley known as the Hazer Jerif, or thousand acres. Like most of the lower Elburz, the Kuh-i-Ja Khan Mura is made of strata of

FIG. 10. Skeleton No. 1 *in situ*.

Jurassic limestone, only slightly tilted. This limestone contains nodules of flint usually pressed between the layers like raised biscuits. All along the edge of the range an abundant rainfall has carved deep gorges, making the border of the plain a scalloped cliff. Owing to the dense vegetation, only where quarrymen have dynamited sections of the face does the rock of this cliff appear.

Oddly enough, it has been established that the Elburz is not, like the Zargros, a part of the Alpo-Himalayan system, which rose at the time of the Villafranchian transgressions, but an older range, whose final folding took place between the Eocene

FIG. 11. Abbas working on skeletons 2 and 3.

FIG. 12. West end north faces.

and Miocene periods.[2] The Kopet Dagh, which runs northwest from the region of Mashhad to the eastern shore of the Caspian, and emerges on the other side as the Caucasus, is Alpo-Himalayan. The more southerly Elburz, which curves slightly southward in its western arc, is not. This fact has a very important bearing on the speleology of the region. Whereas the caves in western Europe, the Zagros, and Kopet Dagh were formed by Pleistocene water action, those in the Elburz may have been formed at any time from the Miocene onward.

In Europe no cave deposits yet found antedate the third interglacial, and most of them start with Würm interstadials. In the Elburz it is theoretically possible to find caves which contain early Pleistocene and even Pliocene remains. However, the level of the Caspian Sea at various periods must also be considered. In order to find such ancient remains we must look for caves which have always been above high water. None of the caves along the seashore of the Kuh-i-Ja Khan Mura fulfill this requirement. The next step should be a thorough exploration of the Nika Valley, and especially the Hazer Jerif country, where such caves have been reported. This will be a horseback trip, since no motor road enters the valley.

[2] Schroeder, J. W., Essai sur la Structure de L'Iran, *Eclogae Geologicae Helvetiae*, 37: 37–81, Basel, 1944.

Neither Hotu nor Belt Cave can contain deposits antedating the highest Caspian flooding, whenever that may have been, since the very top of the Hotu deposit, the higher of the two, was only 18.75 meters above the present level of the flood plain, or 6.75 m. below Black Sea level, and at its maximum flooding the Caspian covered land now shown on the map as being above Black Sea level.[3] However, the Caspian must have flooded more than once, and all floodings may not have been equal. De Morgan found remains of an ancient beach near Behshahr, which is today below sea level, and the same beach seems to appear on the edge of the plain directly in front of our caves.

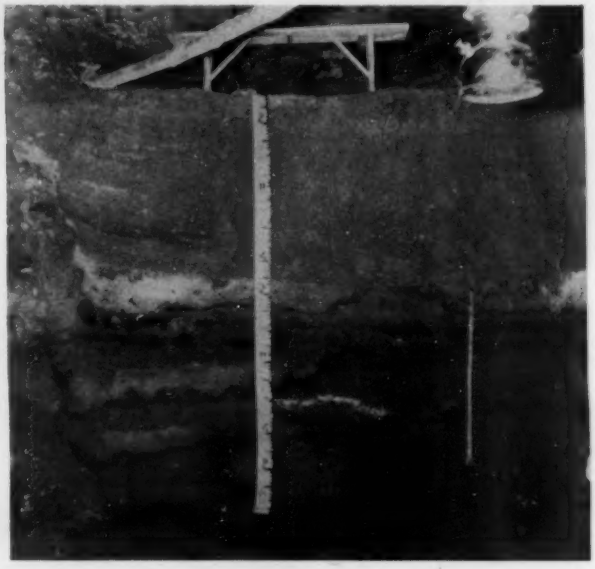

FIG. 13. Details of complex hearth formation, upper Iron Age.

According to Furon (and I translate freely) "One must recall in particular the Würm-Vistula episode of the great quaternary glaciation. The Scandinavian ice-sheet spread far to the south and crossed the divide which separates the Baltic and Volga basins. Afterward the water produced by the melting of the Scandinavian ice sheet flowed into the Aralo-Caspian Basin."

A glance at a map of Russia shows that the watershed between these basins comes to within 300 miles of the Baltic proper, and about 80 miles from the Gulf of Finland. The last retreat of the Scandinavian ice sheet crossed this divide before

[3] Furon, Raymond, Geologie du Plateau Iranien, *Mem. du Muséum National d'Histoire Naturelle*, N. S. 7 (2): 177–414, Paris, Aug., 1941.

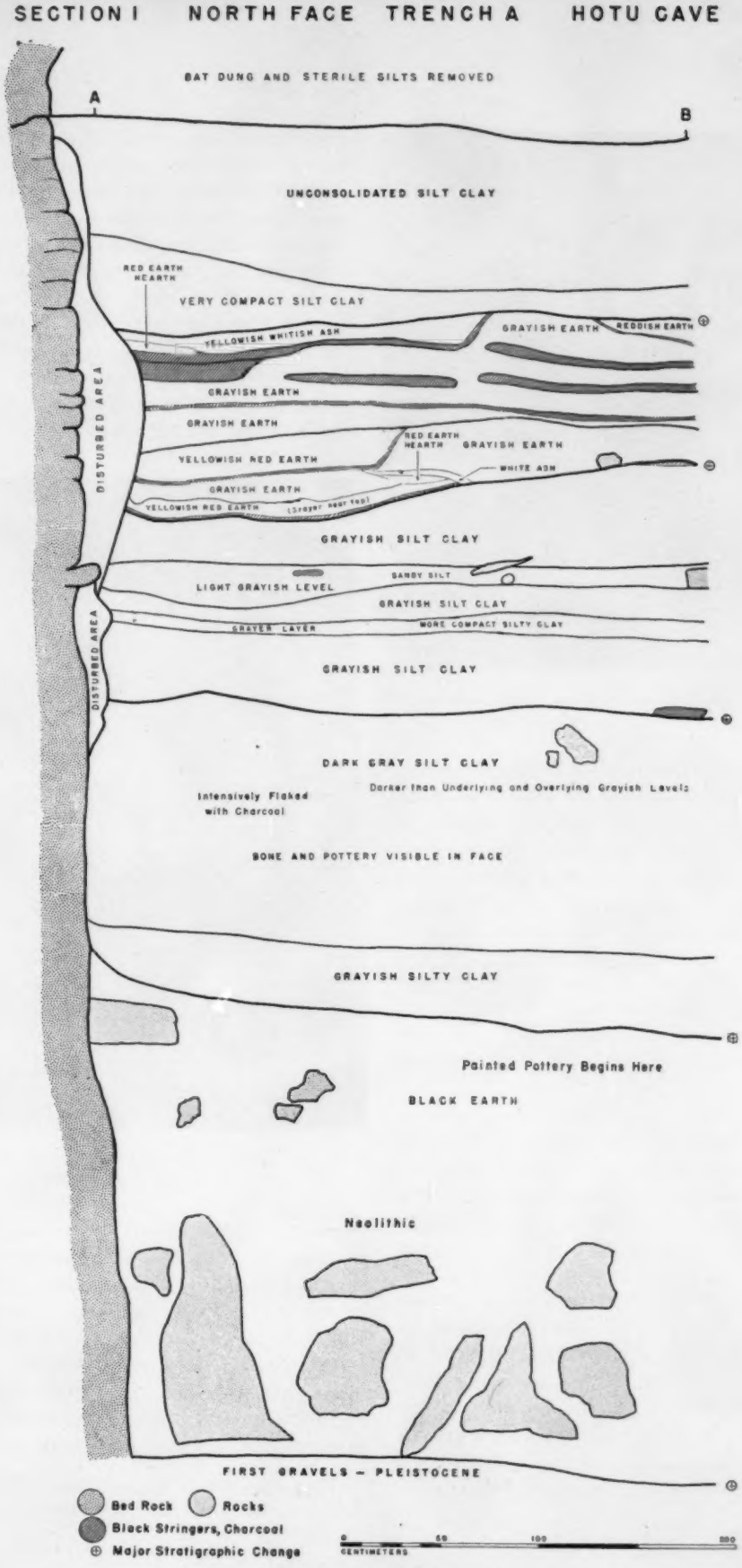

FIG. 14. Section I—North face, Trench A.

FIG. 15. Interior of cave immediately after collapse of north face. Note 3 stranded workmen.

8300 B.C., the initial date of the Finiglacial Period,[4] by which time the Caspian must have reached its highest postglacial level, and the seal hunters of Belt Cave were already at work. Subsequently it sank, reaching a low of 56 meters below present ocean level in the seventh century A.D., after which it gradually rose again to its modern level of minus 26 meters.[5] During the last few years it has again been receding, so that the Ashuradeh Islands have joined the Mian Kaleh Peninsula, and the relatively new port of Bandar Shah has been rendered inaccessible to all but boats of the shallowest draft.

On the basis of Furon's study and other evidence, we may postulate that during full glacial periods, when much of the earth's water was immobilized in ice sheets, the Caspian was relatively dry, and that during the earlier and middle stages of the melting of the Scandinavian ice, the Caspian was relatively full. The deepest portion of the Caspian is in the south where it reaches a maximum of 602 fathoms. This southern basin is separated from the central and northern parts of the sea by a submarine ridge connecting the Caucasus and the Kopet Dagh, and varying in depth from 30 to 150 fathoms. The whole northern third of the sea is very shallow, mostly if not entirely under 50 fathoms in depth.[6] Penck and Antevs variously calculate the rise in ocean sea level which took place during the melting of the last ice at 210 and 272 feet,[7] or 63 and 82 meters. Now the Caspian must have risen more than the ocean, since it received a large portion of the runoff from one side of one of the world's largest ice-caps, and since it is a small self-contained basin, whereas the water that flowed into the ocean had to be shared with such huge bodies as the Atlantic and Pacific. In the Caspian a rise

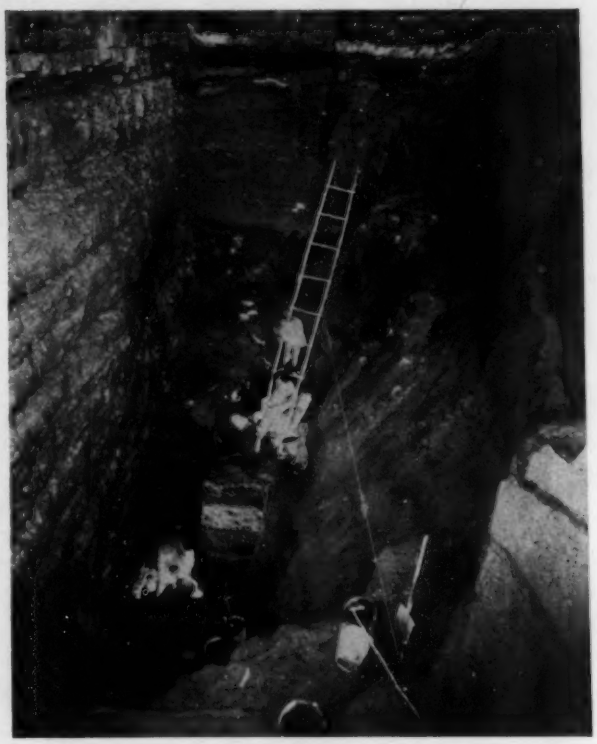

FIG. 16. Cave interior after removal of debris and excavation of Trench D.

of two or three hundred meters may conservatively be expected. A rise of 270 meters would mean that in glacial times the middle and lower sections of the sea were divided into two separate lakes, while the whole north was dry land. A rise of 1,100 meters would mean that the entire Caspian could have been dry at the time of maximum ice. Probably the truth lies somewhere between, that in periods of maximum Scandinavian glaciation,

[4] Clark, J. G. D., The Mesolithic Settlement of Northern Europe, 10–11, map on p. 2, Cambridge, The University Press, 1936.

[5] Furon, R., *op. cit.*

[6] Kropotkin, Prince Peter A., and J. T. Bealby, Article, "Caspian Sea," *Encyclopedia Britannica*, 13th ed.

[7] Clark, *op cit.* 7.

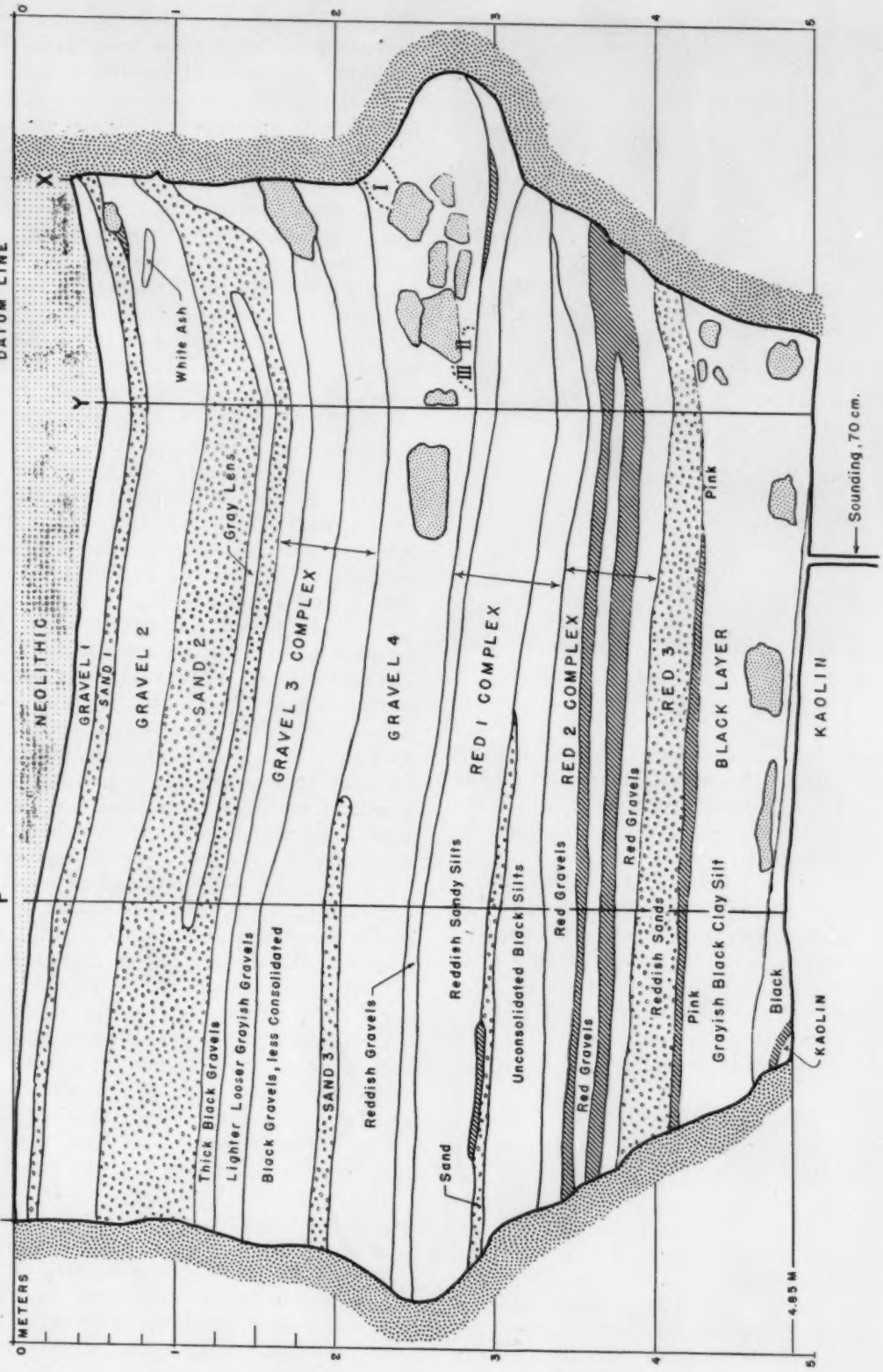

FIG. 17. Section 2—Trench D.

FIG. 18. Trench D showing sequence of gravels and sands.

the Caspian was a pair of lakes, with the southern the larger, and its maximum diameter little more than a half a hundred miles from the mouths of our caves, for the deepest water lies but a short distance to the northwest.

We can postulate, therefore, a situation during the Würm glacial period in which our caves alternately faced or were inundated by the sea, and alternately fronted on a far more extensive plain than exists at present. Sea time is glacial time, plain time is interglacial or interstatial. I have gone into this at some length because it is of vital importance in the complex problem of dating our finds.

3. STRATIGRAPHY

Twelve and a half meters is a deep cut for a cave, particularly for a wet one, in which the relatively stoneless layers are like jelly. Luckily the soft soils extended only the first 7 meters down, and below that the Pleistocene-style gravels held a better face and rendered work underground less hazardous. Before establishing our point A against the north wall we removed a spongy deposit of bat dung and sterile silts, the probable accumulation of several millennia. We excavated this trench in arbitrary levels, about the length of a Persian pick. While these levels varied in depth, 20 or 30 centimeters is a working average. Under it we struck about 80 cm. of unconsolidated silt clay, rich in Iron Age pottery and animal bones. This changed to a compact version of the same, and stopped at a major stratigraphic change at 90 cm. Below this a complex series of hearths and floors, with much ash, charcoal, and different colors of earth, went down to an irregular bottom 1.60 m. below the B point. Between this locus and 2.8 m. we found a second series of silt-clays, over a third major soil change, leading to a deposit of darker-colored silt-clay, which turned lighter as it went down. At 4.60 m. a fourth major soil change was found, under which a single soil extended to the surface of the underlying gravels. Of the 2.25 m. which this soil occupied, the lower half was monopolized by five or six huge slabs of limestone, several meters long and wide, which may have fallen from the roof in wet weather or during an earthquake. In the back half of Trench A these directly overlay the gravels.

Since Mr. Dupree will have more to say about the gravels, I shall merely summarize the evidence. The topmost 2.40 m. consists of an alternation of four black gravels and three layers of sandy soil. In some places the third sand is absent, with the third and fourth gravels touching; in all places the first sand layer is too thin to permit separate excavation. Only the second sand was adequate for separate excavation and specimen bagging. The fourth gravel layer had a private deposit of rockfall, consisting of slabs of limestone about a meter square and 20 or 30 centimeters thick, which seem also to have fallen from the roof. Circumstantial evidence indicates that two of these stones killed the individuals known as Skeletons No. 2 and No. 3, which will be described by Dr. Angel.

Under the fourth gravel the predominant color changes from black to red. Here were again seen gravels, but they are thinner than above, while the intervening layers of sandy and silty soil are thicker. Hearths, similar to the one under skull No. 3, are common. At the very bottom is a layer of grayish black soil, rich and fat to the finger, directly over the apparently sterile kaolin.

Down to the third major stratigraphic change at 2.80 m. the fill along the sidewall of the cave was so loosely packed that actual holes and air-pockets appeared, providing every opportunity for a mixture of specimens among the overlying layers. Below the third soil change where the weight of

the soil above was considerable, the fill met the wall without gaps, and the stratification shows less evidence of disturbance.

The gravels themselves have been intensively studied by Dr. John Miller of State College. In brief, his report states that all of these soils seem to have been laid down by the natural processes of cave deposition. None of the soils were washed in or blown in. Hence the sea did not enter this cave after its first human occupation in the level immediately over the kaolin, nor did any stream flood it, nor prevailing wind dune it. The angular pieces of limestone which characterize the gravels apparently came from the walls and roof of the cave itself, during periods of relatively great rainfall. This is to be expected since the geologically ancient cave deposits in Europe, North Africa, and the Zagros mountains which have been excavated normally contain this same material. Dr. Miller states emphatically that nothing in the nature of the soils themselves gives him the slightest clue to their age. The comparison to early gravels elsewhere is my own.

Before we leave the question of stratigraphy, it may be pointed out that the top of the gravels in Hotu Cave stand within 20 centimeters of the bottom of the Seal Mesolithic in Belt Cave, which was tentatively dated at about 8500 B.C. by Carbon 14. The clays under the Seal Mesolithic were apparently water laid. Belt Cave is closer to the small valley stream than Hotu. None of the culture-bearing deposits in Belt Cave contained the angular limestone chips found in the Hotu gravels.

4. CULTURAL MATERIALS, IRON AGE

In the deposits above the gravels pottery was found throughout. In Trench A we bagged every sherd. In Trench B they were counted, but some were left on the site. In Trench C we were even more selective. After our return to Tehran, Mr. Dupree sorted the Trench A sherds into wares, which he then tabulated by levels, from 1 to 45. The results of this work will be reported later. However, it may be stated here that the fourth major soil change, at 4.60 meters from the top, marked a transition from earlier wares to a sophisticated Metal Age pottery. The sherds of this upper complex have been examined by Dr. Donald McCown, who finds that it resembles Iron Age wares from an undated site known as Lalailou-Tapa in Georgia, as reported by Ghirshman,[8] more than it does any of the Iranian plateau wares. It includes some unusual shapes, such as a flat circular rimmed and perforated plate, held up by a tripod, a thick hollow stem, or a combination of the two. The obvious comparison which it is far too early to risk would of course be with the bronze offertory vessels of China. In this deposit both spouts and handles run through parallel evolutionary sequences. The spouts are open, and the handles in some cases ornamented with animal heads or figures, executed in a style reminiscent of both Luristan and Scythia.

FIG. 19. Horse-shaped handle of Iron Age jug.

Iron, and copper, bronze, or both, are found in nearly all levels of this ceramic horizon. Where they are absent it is probably due to the general scarcity of metal in the cave. The most handsome object was a bronze or copper javelin head, in perfect condition, which the Tehran Museum kept. Various kinds of pins were also found, and a small square of sheet copper perforated as if for sewing to a garment. One ornament which turned up only after chemical treatment is a copper spiral surrounding a rod of iron. Mr. Parkinson, our staff chemist, has been unable to study the copper and/or bronze objects to see which they are because of their scarcity and our reluctance to lose them in the process. It is hard to say what the iron objects were meant to be, so badly are they rusted. Although animal bones from these levels have not even been unbagged, field notes indicate the pres-

[8] Ghirshman, R. *Fouilles de Sialk*, Musée du Louvre, Série Archeologique, **5** (2) : 87–88, pl. CL, Paris, 1939.

FIG. 20. Sherds of painted pottery of Sialk II type.

ence of ox, pig, sheep, goat, and red deer throughout.

This Iron Age material seems to indicate a homogeneous, locally evolving culture, which must have ended by 600 or 700 B.C., when the mouth of the cave was sealed by the accumulation of soil sliding down the hill through a crack shown in both plan and photographs. This terminal date is suggested by Dr. McCown because of the complete absence of Achamenian or later sherds in the collection. The initial date is more difficult to discover. Dr. McCown, on his way to Iraq, conservatively set it at about 1000 B.C. Soon afterward Dr. William Libby and Miss E. Ralph obtained a date of 4670 ± 230 years, or 2490–2950 B.C., by testing a large sample of charcoal from the 4.00–4.60 meter level in Trench B. While in Trench A we cut through the fourth major soil change in Level 31, in Trench B we were looking for it, and were able to trace the transition line westward from its position on the south face of Trench A. The chance that we mixed charcoal from below the soil change line into the 4.00–4.60 meter sample is negligible. Miss Ralph warns that a mistake can have been made in the laboratory, where the counting was done with new equipment recently installed, and she intends to test this sample again once her laboratory shall have been set up in the University Museum.

However, I am not prepared to reject offhand a third millennium date for a metal age site containing both copper or bronze and iron, since iron implements have been found sporadically at a comparable period in both Egypt and Mesopotamia. However, to my knowledge neither Egyptian nor Mesopotamian sites contain slag before the Iron Age proper, around the conventional date of 1000 B.C. The Hotu deposit *does* contain slag. It may also be mentioned that one of the chief industries of Mazandaran is charcoal burning. Not only is fuel abundant there, but so is ochre, which may not have come from very far away. It also seems odd that even with the cooperation of bats 4.60 meters of cave soil, marked by three major stratigraphic changes, could have been deposited, by people who did not actually live in caves, during only three hundred years.

5. CULTURAL MATERIALS, NEOLITHIC AND PAINTED POTTERY

While the Iron Age took up 31 levels in Trench A, the underlying ceramic deposit accounted for only 14, from No. 32 to No. 45. This relatively homogeneous lower soil layer averages about 2.50 m. in thickness, little over half that of the Iron Age. In it a Neolithic software exactly like that previously discovered in Belt Cave appears to evolve into a thin walled black-on-red painted pot-

FIG. 21. Neolithic level reversed stone bowl *in situ*.

TABLE 1

HOTU CAVE - PLEISTOCENE MAMMALS

	Bos sp. or primigenius	Cervus elaphus	Ovis sp.	Gazella subgutturosa	Sus	Canidae	Vulpes	Phoca	Mustela nivalis	Parechinus (?)	Ellobius	Microtus socialis	Rhombomys opimus	Citellus fulvus	Myotis myotis	Total
Gravel 1	7		10			1	1	1							1	21
Gravel 2	13	18	49		3	6	4									93
Sand 2		1	20	4	1	1	1				1					29
Gravel 3	5	1	28	2	2	1			1		11			1		52
Gravel 4	1		3		1	1					7		3	2		18
Red Gravel 1										1	10	1		1		13
Red Gravel 2			2	3		1		2			1	1				10
Red Gravel 3			2	2				1								5
Red Sand over Black				3												3
Bottom Black	1															1
Total	27	20	114	14	7	11	6	4	1	1	30	2	3	4	1	245

tery, which Dr. McCown relates to Sialk II on the plateau. Intermediate stages are seen when the software acquires a burnished red surface, on which lines are drawn; then the thinware appears, along with the soft, and the typical geometric designs are established. At the 4.60 m. soil change, both software and painted pottery stop as if cut off with a knife.

No metal at all was found in this soil. Since cutting and piercing tools of flint, bone, and polished stone yield an adequate toolkit, there is no visible gap which metal could have filled. The usual domestic animals are present, but the ox bones become scarcer as one goes downward, and are absent near the bottom. One suspects that it was during this period that the ox was either domesticated or introduced, shortly after the pig. Sheep and goats were apparently present as domestic animals throughout. These findings support the evidence of a purely software Neolithic horizon in Belt Cave, in which domestic sheep and goats were found, but pig and ox came in only in the mixed upper layers. No pre-ceramic Neolithic, confirmed for Belt Cave, was found in Hotu. It would seem that when the Neolithic people left Belt they moved into Hotu, previously unoccupied since the Palaeolithic. When they left Hotu some of them may have gone to the plateau, bearing their painted pottery to Sialk. On the basis of his extensive knowledge of Middle Eastern pottery, Dr. McCown tentatively assigns the pre-metal age ceramic horizon of Hotu to the entire fifth millennium B.C., a date which the Belt Cave Carbon 14 findings support.

FIG. 22. Humerus *bos primigenus* found at very bottom of deposit of Trench D.

6. THE PLEISTOCENE FAUNA AND ITS IMPLICATIONS

This paper sets the stage for the location in time and climate of the human beings whose battered remains we uncovered in the gravels and the tools they left behind. More technical studies are given by Dr. Angel and Mr. Dupree in the papers which follow. On the basis of experience with the Belt Cave faunal material I concentrate on the study of the animal bones recovered from the rather complicated levels of which this deposit consists. Despite the small size of our trench, the number of bones recovered was great. Out of well over 1,000 pieces, some 245 have been identified to date, partly by Dr. F. C. Fraser of the British Museum, partly by Mr. Fred Ulmer of the Philadelphia Zoo, and partly by myself on the basis of key bones identified earlier, for the Belt Cave, by Dr. Henry Setzer of the Smithsonian. I confined my own activities to easy genera, like sheep, goat, gazelle, pig, and seal. Dr. Frazer's chief contribution lay in the realm of the wild ox and carnivores; Mr. Ulmer's in that of rodents, bats, weasels, foxes, and other small game.

Table 1 shows the results of our combined efforts so far. It is a tentative table, since more may be added, and my own identifications must be checked. However, for present purposes it seems adequate. Over a third of all specimens came from the second gravels. Only 13 per cent represent the entire red sequence, and the bottom black layer yielded but a single specimen, the proximal end of the humerus of a *Bos primigenius* which impressed even Dr. Fraser, who is used to big oxen, with its enormous size. Bypassing the second sand layer, we see that Gravels 1 through 4 account for 75 per cent of all our bones.

The ox was hunted in black-gravel times, as well as at the very beginning. The red deer is almost exclusively a second-gravel animal, and the single specimen from the second sand may well have belonged to either side. It is hard to say the same for the twenty specimens of sheep; this animal, apparently indistinguishable from the domestic variety found higher up in Hotu and in Belt Cave, runs through our whole sequence with complete impartiality. *Gazella subgutturosa,* on the other hand, is more particular, selecting the red sequence, and the second sands. With little question the two specimens in gravel three are linked with the second sands. Pig is a black gravel animal, while dogs, jackals, and foxes show no particular association. The seal, on the other hand, belongs with the lower or red gravels, while the one specimen in the topmost layer may well have been deposited on the surface during the time-gap between the end of the gravel occupation and the beginning of the Neolithic habitation. The one weasel in Gravel 3 has no apparent significance, but the hedgehog in Red Gravel 1 has. Mr. Ulmer states that it is definitely not *Hemiechinus* nor *Erinaceus;* his identification as *Parechinus* is tentative owing to the lack of a specimen in the local collections. In any case it is definitely a desert animal.

Now *Ellobius* sp., the mole-vole, is also a drylands animal. Like the mole it spends its life burrowing just under the surface, but using its incisor teeth instead of its forepaws. The ground-squirrel (*Citellus fulvus*), the gerbil (*Rhombomys opimus*), and a meadow-mouse (*Microtus socialis*), who despite his English name is known to inhabit dry places, are also desert and grassland fauna. The one bat (*Myotis myotis*) was identified by a single jaw; many bat long-bones which are not included here were found in most levels. Since the bat has probably inhabited the cave ever since its formation or since bats began, whichever was earlier, little significance can be attached to its presence other than as a source of

dung, thus making it partly responsible for filling up the cave.

On table 2 this information is summarized. For reasons which Mr. Dupree will explain, it has been decided to lump Gravels 1 and 2, without reference to the first sand layer, which was very thin, which seems to have carried no special fauna, and was possibly sterile. The abundance of limestone chips in these and in Gravels 3 and 4 would suggest a relatively wet climate. The ox, red deer, and pig are forest animals. The sheep is a mountain animal, and mountains are near.

However, the gazelle, found in the second sands, is a desert or steppe animal. This would indicate that at least once a drier climatic phase intervened between two wet phases, marked by Gravels 2 and 3. The red gravels, however, also contain gazelle bones, while the ox, deer, and pig are absent. The presence of sheep would indicate that an arid plain at this time approached the mountains. Three unmistakable seal bones in Red Gravels 2 and 3 also show that the Caspian shore could not have been far away. The evidence of aridity is confirmed by the quantity of mole-voles and associated vermin in the upper red gravels, and continuing into Gravels 4 and 3.

To conclude this picture I sent 118 presumably identifiable bird bones to the long-suffering Secretary of the Smithsonian Institution, Dr. Alexander Wetmore, who has laid them on his already overburdened table. Of the 118 only 11 came from the two top gravels, none from the second sands, 31 from Gravels 3, 22 from the fourth gravels, 49 from Red Gravels 1, six from Red Gravels 2, and one from the black under the red gravels. Dr. Wetmore authorizes me to say that "the bird material includes remains of a number of partridge-like birds of the area, pigeons, a small owl, and a number of song birds, including thrushes and several other allied species of slightly smaller size. Presumably these were used as food."

Quantitatively speaking, the majority of the bones sent to Dr. Wetmore are those of small birds. The distribution of bird bones in the levels, with 47 per cent in the red series, 45 per cent in Gravels 3 and 4, and only 8 per cent in the two upper layers, parallels that of *Ellobius*. Dr. Wetmore's observation that the birds were eaten is confirmed by the fact that many are charred. The same is true of the vole bones. The three vole skulls had all been broken behind the orbits, apparently to get at this little creature's succulent brain.

TABLE 2

PLEISTOCENE FAUNA
WET & DRY FORMS

	Ox, Deer, Pigs	Gazelles	Sheep	Dogs, Jackals, Foxes	Seals	Weasel	Rodents & Hedgehog	Bat	Total
Gravels 1 & 2	41		59	12	1			1	114
Gravels 3, 4, and Bottom	11	2	31	2		1	24		71
Sands and Red Gravels	2	12	24	3	3		16		60
Total	54	14	114	17	4	1	40	1	245

	Wet[1]	Dry[2]	Complacent[3]	Total
Gravels 1 & 2	41	0	73	114
Gravels 3, 4, and Bottom	11	26	34	71
Sands and Red Gravels	2	28	30	60
Total	54	54	137	245

[1] Ox, Deer, Pig
[2] Gazelle, Rodents, Hedgehog
[3] Sheep, Dog, Jackal, Fox, Seal, Weasel, Bat

Coming back to table 2, the lower section is arranged in such a way that definitely wet-living mammals are grouped together, and dry as well. Animals which can live in either kind of country, in the fringing mountains, or, like the seal, in neither, are lumped under "complacent." It is clear that a very high order of correlation is present, with four chi-square values running from 9.7 to 25.6.[9] The red series and the second sands are dry-fauna deposits; the two top gravels are wet fauna deposits, while Gravels 3 and 4 fall in between. The small birds go with the dry fauna and with the intermediate fauna of Gravels 3 and 4. Culturally this means that the people who lived in Hotu before, during, and after the time of our three human specimens depended more on gathering slow game and robbing birds' nests than on hunting. Their food-sources indicate a life of dryland walkabouts, like some of the Indians of the American Great Basin, and Lower California. An occasional trip to the shore would give them a chance to club a few seals. The later inhabitants,

[9] Top gravels and wet = 9.9; sands and R. G. and wet = 9.7; top and dry = 25.6; sands and R. G. and wet = 17.1.

as shown by the two upper gravels, were forest and mountain hunters, concentrating on wild oxen, red deer, and sheep.

Aside from this cultural differentiation, the climatic evidence which the fauna yields fits in well with the geological data given in section 2. The key animal is the seal. When seals were eaten, the Caspian shore was near. When they were not, one may presume that the sea was shrunken or dry. Now the seal goes with the dry land fauna, particularly the gazelle. This would indicate a time or series of times when the Scandinavian icecap had melted, the Caspian was full, and the Turkestan steppe had invaded the southern Caspian shore. An interstadial or interglacial, or some other kind of oscillation, is suggested. The ox, red deer, and pig, however, need forests, and forests need rainfall. In the time of Gravels 3 and 4, the forest was perhaps encroaching on the steppe, where the voles were still to be unearthed. In Gravels 1 and 2, the forest had pushed out the steppe, and the voles were gone. No seals were at hand. Perhaps the icecap had once more formed. However, between the times represented by the third and second gravels, another dry period had intervened, and it is possible that the thin layers of Sands 1 and 3 mean two other and briefer fluctuations. Except for the one bone in the top of the first gravel, no seals are to be seen; however, in Trench A, in a level which could have been the second sands or second gravels, one very significant object appears—the lower jaw of a fish, *Rutilus rutilus caspicus jacovlew,* the Caspian rudd. Rudd jaws are as distinctive as gazelle horns. The fish is still there, and the Caspian is its only home.

The only conclusions which can be drawn from this study are that the very first occupation of Hotu Cave, above the kaolin level, took place in a wet period. Then the climate grew drier, and the southern shore of the Caspian Sea occupied more or less its present position, for a considerable period. After this the prevailing climate was again wet, with at least one and possibly three reversions to the drier state. Of the four gravels in the later or wetter series, the lower two were less wet than the last two. In the most extensive (or only) dry interval, the Caspian may again have been filled. After all of this had happened the Mesolithic deposits of Belt Cave were laid down, with or without a time gap.

How to fit all this into a coherent geological picture is beyond my capacity. The presence of a Scandinavian icecap is predicated on the absence of seals and rudd, a negative piece of evidence. The presence of seals and rudd, however, being positive evidence, clearly suggests the absence of an icecap during the alternate periods. The coincidence of the presence of the sea with drought shows a situation quite different from that existing today, when we have both the sea and wet conditions in eastern Mazandaran. In the lowest cultural level of Belt Cave, the sea was there and the climate was wet, as today. In the second cultural level from the bottom, the sea was there and it was dry. From Neolithic times on, it is probable that conditions had reverted to the combination of sea and wet which have clung on until the present.

Since no one knows how long it took for all of these shifts of climate and fauna to take place, all I can say is that, since this sequence is clearly older than that in Belt Cave, we are dealing with some complicated local climatic pattern during all or part of Würm time. The problem is not insoluble. We are not even dependent on future geological work in the Elburz Mountains, for the shifts in level of the Caspian, our key to the picture, are based not on Elburz or Caucasian but on Scandinavian glaciation.

A final note of complication if not confusion is injected by the kind efforts of Dr. R. Olsen of the Museum of Comparative Zoölogy at Harvard, as shown in table 3.

Owing to the shortage of time we sent Dr. Olsen only these seven pieces of bone, three human and four ungulate. Unfortunately these bones had been treated with alvar, which Dr. Olsen removed, presumably with acetone. He writes:

> The results are given in percentage weights of the bone freed from alvar. Most of these specimens clearly retained organic matter. Number one, for ex-

TABLE 3

FLUORINE CONTENT, SIX SPECIMENS

	Fluorine, %	No. of determinations	No. of titurations
Gravels 1	.01	1	2
Gravels 2	.65	3	6
Gravels 3	.01	1	2
Gravels 4	.02	1	2
Skeleton No. 1	.27	1	2
Skeleton No. 1 (second)	.11		
Skeleton No. 2 or 3	.06	1	2

ample, underwent a 14 per cent loss of weight when heated sufficiently to drive off volatile constituents. At a future date it will be advisable to check most specimens for phosphate content. At the moment, however, I believe I can assure you that no correction of that sort will affect the relative significance of the figures as here listed.

If we skip the pieces from the second gravels and first skeleton, we see a negative picture. Not enough fluorine accumulated in the remaining four specimens to give any indication of relative age. However, the one bone out of several hundreds in the second gravels which I chose by chance to send to Harvard turned up an exotically high fluorine count. Dr. Olsen says:

> Please note that the entire analysis was run three times on specimens H-D Gravels 2, and agreement was complete. . . . Conclusive, however, is the case of fragments from Gravel 2. The fluorine value is relatively so much higher that in my opinion either of the two following conclusions may be drawn: (1) the bone is significantly older; (2) it originated elsewhere and is intrusively present at that level.

Dr. Miller, on the other hand, stands Horatius-like in his opposition to the intrusion theory. Some feckless hunter's child may have brought in an old bone in the time of Gravel 2, but its chance of being selected for Dr. Olsen's scrutiny out of the hundreds of other bones in that deposit is infinitesimal. I am happy to stand on the sidelines during this controversy, the first round of which can only be settled, as Dr. Olsen himself states, by the analysis of more bone from this level.

The second round will be more geology, with more excavation; the third, a final examination of the validity of the fluorine method, and the fourth, Carbon 14. If skeleton No. 1 was tied in a bundle and leaned against the wall earlier than the fall of rock which pinned down skeletons 2 and 3, then something exceedingly curious must have happened. Skeleton No. 1, however, does not represent an accident or a fresh interment. It was a secondary burial of the spare parts of several individuals. Does this mean that the successors of skeletons 2 and 3 happened to discover some very old bones lying around in some forgotten spot, perhaps in another grotto, collected them, packaged them, and brought them into their own cave to lean against the wall? Or does it mean that the fluorine method requires some correction of which we are at present ignorant?

The only examples of the use of fluorine tests on early man with which I am acquainted are those

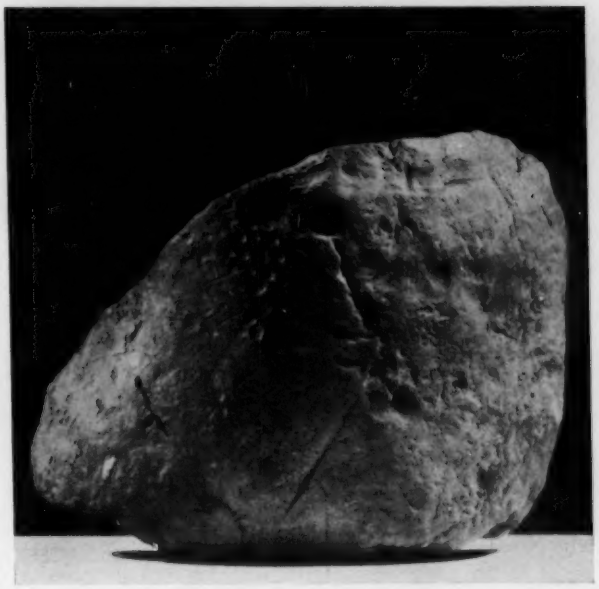

FIG. 23. Lump of fired clay showing textile or cordage marks.

of Oakley and Hoskins on Piltdown, and Oakley and Ashley Montagu on Swanscombe. Ten specimens of Piltdown, by morphological standards an exceedingly ancient type of human being, averaged .20 per cent fluorine, with a range of .1 to .4 per cent.[10]

The mean is less than the fluorine content for our skeleton No. 1, and comparable to that for five specimens of "Holocene or Pleistocene" bone from the same site, Piltdown. The "Middle or Upper Pleistocene" animal bones yielded .68 per cent, and the lower Pleistocene 2.42 per cent. As Oakley points out, the latest period to which Piltdown can be assigned is the Riss-Würm or Third Interglacial, the last time that the associated warm fauna, particularly hippopotamus, which is peculiarly sensitive to frozen rivers, lived in England. Yet the fluorine content is too low for that date.

On the basis of a higher fluorine content than that of Piltdown, .34 per cent, Oakley and Ashley Montagu rejected the alleged remote antiquity of the Galley Hill skeleton,[11] because the local human and other fauna ran to .15 per cent for Roman and

[10] Oakley, K. P., and C. R. Hoskins, New Evidence on the Antiquity of Piltdown Man, *Nature* **165**, March 11, 1950.

[11] Oakley, K. P., and M. F. Ashley Montagu, A Reconsideration of the Galley Hill Skeleton, *Bull. British Museum (Nat. Hist.), Geology* **1** (2), 1949.

Saxon times, 1.10 for the Upper Pleistocene, and 2.02 per cent for the Middle Pleistocene. To this earliest stage the Swanscombe skull, with 2 per cent, clearly belonged.

The fluorine test is admittedly relative. Some regions invite fluorine penetration, others do not. I question whether or not this regional differentiation may not extend to tiny regions, such as the different loci in a single cave, in which certain bones from a higher level may also be more fossilized than certain others from a lower level, depending on what water strikes which, and in what quantity. The Neanderthaloid maxilla [12] found in the High Cave of Tangier in 1939 was still incompletely fossilized, while some animal bones from higher up rang like a bell when struck. If this can happen to mineralization, cannot the fluorine quantity vary in a similar fashion? Skeleton No. 1 was leaning against the wall of the cave, right in the path of seepage, while No. 2 and No. 3 were out in midstream.

My reaction to the fluorine tests so far made is that they raise new problems and settle no old ones. In this sense they may serve the useful purpose of keeping us even further from complacency and premature decision than before. I do not know how long ago the vole and thrush eaters of Hotu Cave lived, and I do not expect to know for some time.

APPENDIX

SOIL SAMPLES FROM HOTU [13]

JOHN P. MILLER

Pennsylvania State College

TRENCH D

1st Gravels. About 20 per cent is angular white or gray limestone up to one inch; also a few fragments of chert and sandstone. Remainder is mostly gray calcareous fine sand.

2nd Gravels. About 60 per cent gray to buff angular gravels up to 1–1½" diameter. Mostly limestone with some pink quartzite and red sandstone. Remainder is sand and silt. Dark due to presence of charcoal. Tiny vertebrate bones also present.

2nd Sands. Approximately 75 per cent is fine calcareous sand or silt with a small amount of charcoal. Remainder is coarser material with limestone pebbles up to one centimeter.

3rd Gravels. Approximately 50 per cent is angular fragments up to 2" diameter. Mostly gray to buff limestone, but including some light-colored chert, which is chipped. Remainder is sand and silt composed mostly of limestone fragments of angular shape; also some charcoal. No apparent difference from the second gravels.

From the third gravels were also taken pieces of shaly, white to reddish limestone, very angular, and up to 6" diameter. Apparently this is material which fell off the cave roof.

4th Gravels. About half is gray or white angular fragments of limestone and limey sandstone. The remainder is smaller fragments of limestone with some fine calcareous sand and silt. Charcoal is present, and there are a few tiny vertebrate fossils.

Red 2, Blackish silt. Composed mostly of silt and fine sand containing a very high per cent of charcoal. Fraction greater than 40 mesh includes pebbles up to one cm. which are mostly limestone. This sample also contains rootlike calcareous fossils.

Red 3, Reddish earth. Slightly calcareous sand, silt, and clay. Dominantly angular to sub-rounded quartz coated with red clay. Charcoal abundant. Particles greater than 40 mesh are as large as 18 mm., mostly limestone but includes some quartzite and red sandstone. A peculiar tiny calcareous fossil, unidentified, looking rather like the ends of plant roots, is present in great abundance.

Pinkish clay in same complex is composed mostly of silt and clay, but includes also some sand and is slightly calcareous. Mineral matter is mostly quartz with a smaller percentage of charcoal. Fraction greater than 40 mesh includes particles up to 10 mm. and is composed of limestone with some red siltstone. Vertebrate bones also present.

Black layer. Grayish Clay in corner of trench near bottom. About 75 per cent fine sand and silt; calcareous; contains some pebbles up to 10 mm. Charcoal is present and there are numerous vertebrate bones.

Underlying kaolin. White clay, made up entirely of calcium carbonate; that is, practically the

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[12] Şenyürek, Muzaffer Süleyman, Fossil man in Tangier, *Pap. Peabody Mus. Amer. Archaeol. & Ethnol.* **16** (3): 3–8, 1940.

[13] Dr. Miller, who was trained under the late Dr. Kirk Bryan of Harvard, made a technical study of the Belt and Hotu soil samples immediately after their arrival in America. Although his report covered both caves and all principal levels, I have edited it to deal only with the pre-Neolithic gravels and accompanying soils of Hotu. C. S. C.

entire sample is soluble in HCl. Consists of angular fragments of white to buff limestone up to 15 mm., with finer calcium carbonate down to silt sizes, which forms a matrix for the larger fragments. There are a few pebbles of calcareous sandstone which leaves, on HCl treatment, a residue of poorly sorted sub-rounded quartz and chert grains. A small amount of charcoal is present and there are some vertebrate forms. This sample probably represents a different formation from the one which seems to predominate in the same deposits.

DISCUSSION OF SAMPLES

1. The materials from both Hotu and Belt caves probably are rather normal cave deposits; that is, they are formed by rock falls from the roof and walls of the limestone cave, and also from materials carried downward by ground water and also weathered from the limestone. This explains the mixture of angular limestone fragments with silt and clay-sized materials. For some unknown reason soil materials in Belt are much better sorted than those in Hotu.

2. It is not impossible that some of the fine-grained material is wind-blown, though it is very difficult to prove from this set of samples. Wind-blown materials characteristically are very well sorted and commonly show frosting and pitting of individual grains. Furthermore, the grains of wind-blown materials are generally rounded. Because none of the Hotu or Belt materials show any of these characteristics, I seriously doubt that any of this material is actually wind-blown. Of course, it could have been blown into the cave from a source so nearby that there was not time for development of the characteristics just mentioned.

3. I see no reason to believe that there are any water-laid deposits in either of these caves. Here again, materials carried by streams show rounding and reasonably good sorting. These cave deposits, as already mentioned, show neither of these characteristics. Certainly the "gravels" which you have described have not been transported by a stream.

I am assuming, in making these statements, that both Hotu and Belt are normal limestone caverns rather than caves formed by collapse in the face of a limestone cliff. If they are caverns, then the materials I have examined look very much like the cave deposits that I have seen at various places here in the United States; and it is primarily for this reason that I ascribe the origin of these deposits to normal cave processes rather than to wind or stream action.

So far as I can tell, there is no basis for dating the soil samples, and hence the artifacts from the soil samples. Any dating that is done will have to depend on other techniques, such as carbon 14, the artifacts present, and anything else that seems possible.

[Dr. Miller also expresses his doubt that any of the soils which he examined are earlier than the last glaciation. C. S. C.]

THE PLEISTOCENE ARTIFACTS OF HOTU CAVE, IRAN

LOUIS B. DUPREE

Field Assistant, University Museum Iranian Expedition 1951, University of Pennsylvania

(*Read November 8, 1951*)

THE FLINT ARTIFACTS

A TOTAL of 2,874 pieces of flint were collected in the Pleistocene levels, of which 132, or 4.6 per cent of the total number were cores and core tools; 2,625, or 93.1 per cent of the total number, were flakes and flake tools; and only 117, or 4.1 per cent of the total number, were blades and blade tools. Out of the total of 2,874, there were 330 implements, 11.5 per cent.

The cores and core tools include: 17 core scrapers; 5 keeled scrapers; 5 micro-cores; 14 hammerstones; 2 "hand axe" types with triangular cross-sections; and a large chopper.

The striking platforms of the cores, from which flakes or blades were struck, are all plain; and none of the cores have more than one striking platform. Incidentally, no cores were found below the Fourth Gravels until we reached Red Gravels III, and there we uncovered six.

Out of the 2,625 flakes and flake tools, 229 were actually utilized or retouched as implements, or 8.95 per cent of the total flakes. No true Levallois flakes, so diagnostic of the Mousterian or Middle Palaeolithic Period of Europe, were found at Hotu.

TABLE 1

CORES AND CORE TOOLS — HOTU CAVE, MAZANDARAN, IRAN	Unutilized Cores	Core Scrapers	Keeled Scrapers	Micro-cores	"Hand axe" Types	Chopper	Hammerstones	Totals
First Gravels	35	9	1	1	2	0	2	50
Second Gravels	14	4	2	2	0	1	2	25
Second Sands	5	2	2	0	0	0	3	12
Third Gravels	24	2	0	2	0	0	3	31
Fourth Gravels	4	2	0	0	0	0	4	10
Red Gravel I	0	0	0	0	0	0	0	0
Red Gravel II	0	0	0	0	0	0	0	0
Black Soil under Red Gravel II	0	0	0	0	0	0	0	0
Red Gravel III	6	0	0	0	0	0	0	6
Black Soil under Red Gravel III	0	0	0	0	0	0	0	0
Totals	88	19	5	5	2	1	14	134
Percentage of Individual types to Total Cores	*65.7*	*14.2*	*3.7*	*3.7*	*2.2*		*10.4*	

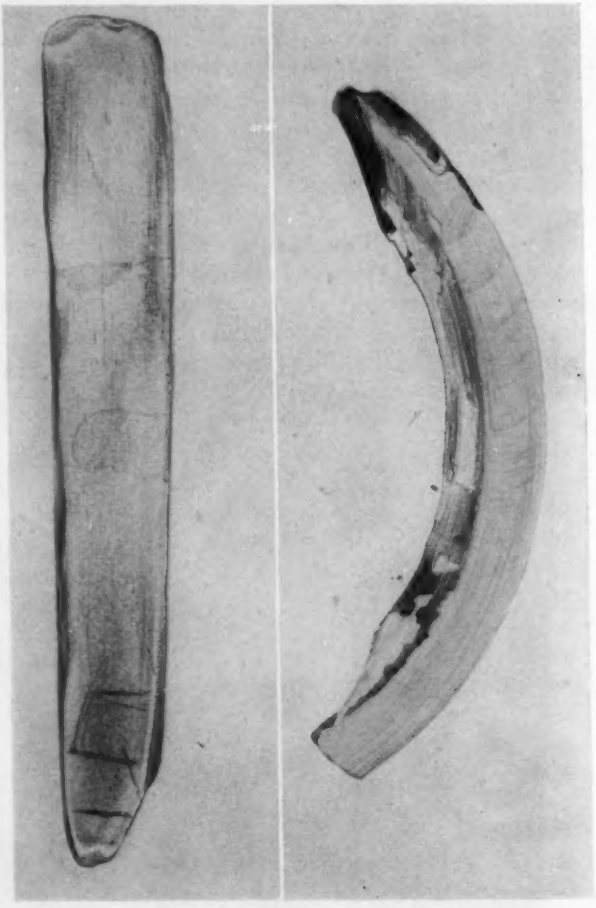
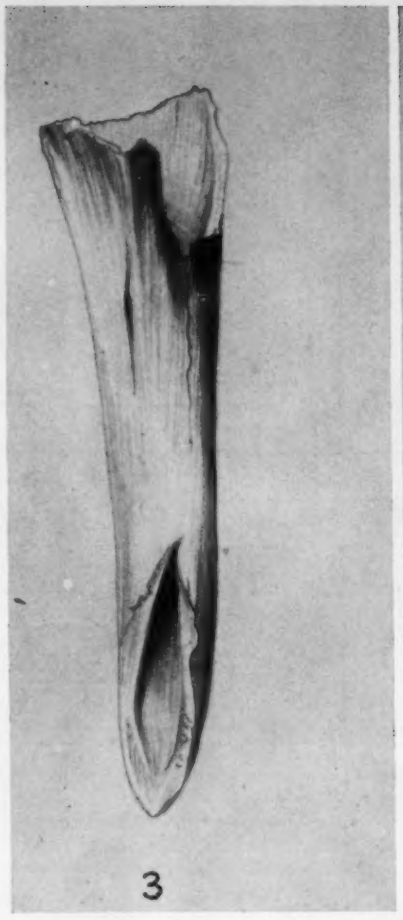
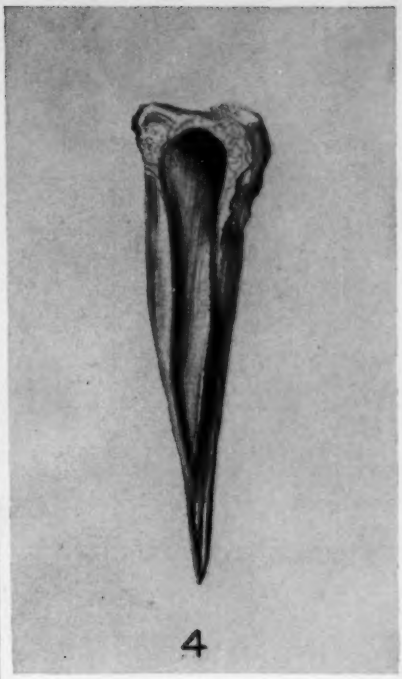
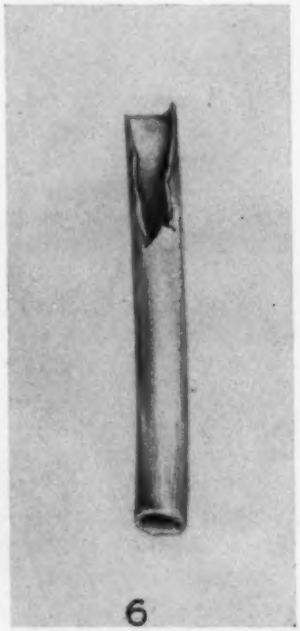


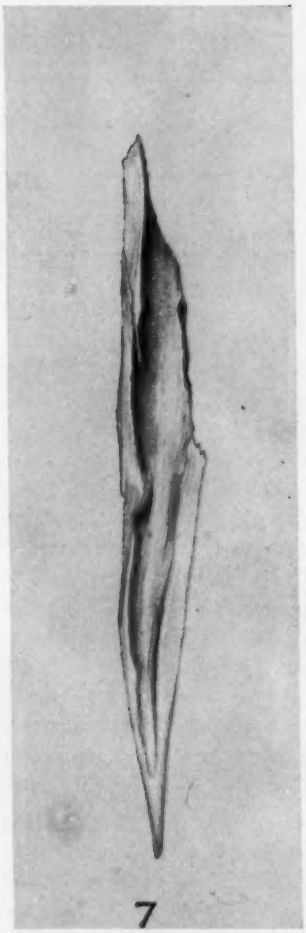
FIG. 1. Bone knife with "S" incision, Trench A Level 49.

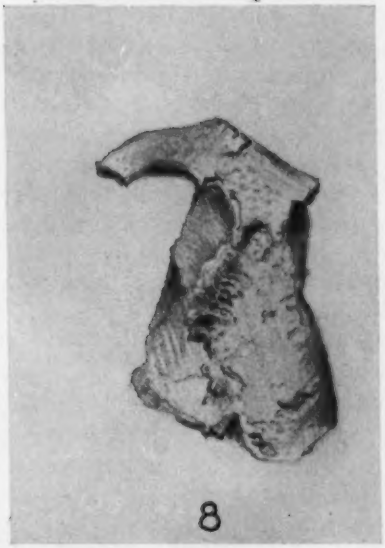
FIG. 2. Curved knife made of boar's tusk, Trench D 1st gravels.











FIGS. 3, 4, 7. Awls. 1st gravels Trench D, 3rd gravels Trench D, 3rd gravels Trench D. 5. Needle, 1st sands Trench D. 6. Tube, Trench A Level 52. 8. Clay figurine, Trench D 3rd gravels.

TABLE 2

FLAKES AND FLAKE TOOLS — HOTU CAVE, MAZANDARAN, IRAN	Primary Flakes (Core Trimming)	Core Rejuvenating Flakes	Flake-Blades, Single Edge (Knives or Side Scrapers)	Flake-Blades, Double Edge	Knotched Scrapers	End Scrapers	Oblique End Scrapers	Discoidal Scrapers	Drills, Awls, Perforators	Borers ("Half-moon" Shape)	Spoke Shave-type Scraper	Retouched Flake Butts "Pseudo Levallois Flaking"	Points (?)	Combination End and Side Scraper	Burins	"Pseudo-Burins"	Amorphous Pebble and Flake tools	Totals
First Gravels	794	1	8	0	2	2	0	5	6	0	4	5	1	0	2	8	32	870
Second Gravels	383	3	8	0	4	1	5	0	4	2	5	1	1	0	0	4	29	450
Second Sands	146	3	4	0	1	0	2	2	1	1	1	3	0	0	1	2	15	182
Third Gravels	670	1	2	3	0	1	2	1	1	0	5	3	2	0	0	3	6	700
Fourth Gravels	118	1	1	0	0	0	1	0	1	2	0	1	1	1	0	1	2	130
Red Gravel I	43	0	2	0	0	0	0	0	0	0	0	0	0	0	0	0	1	46
Red Gravel II	29	0	1	0	0	0	0	0	0	0	0	0	0	0	0	0	0	30
Black Soil under Red Gravel II	4	0	1	0	0	0	1	0	0	0	0	0	0	0	0	0	2	8
Red Gravel III	159	0	0	0	1	0	0	1	2	0	0	0	1	0	0	0	1	165
Black Soil under Red Gravel III	41	0	0	0	0	0	1	0	0	0	0	0	0	0	0	0	2	44
Totals	2,387	9	27	3	8	4	12	9	15	5	15	13	6	1	3	18	90	2,625
Percentage of Individual types to Total Worked Flakes			*11.9*	*1.3*	*3.5*	*1.7*	*5.2*	*3.9*	*6.7*	*2.2*	*6.7*	*5.7*	*2.6*	*0.4*	*1.3*	*7.9*	*39*	

Total flakes = 2,625
Total unworked flakes = 2,396
Total worked flakes = 229 (*8.5%* of total flakes)

Three flake-blades, marginally retouched on opposing edges, were probably used as knives or side scrapers; all were found in the Third Gravels. However, 27 flake-blades were found which had retouching on one edge only; their distribution extended throughout the Pleistocene levels.

Other flake tools can be summarized as follows: 8 knotched scrapers found in varying levels; 4 end scrapers found in the upper three gravels; 12 oblique end scrapers; 9 discoidal scrapers; 15 perforators; 5 peculiar, "half-moon" shaped borers in the first four gravels; one combination end and side scraper on a flake; 15 spokeshave scrapers; and only six crude points. These points should probably be included with the perforators.

The two most important catagories of flake implements are "burins," and amorphous flake and pebble tools; I might also add a third catagory in quotes—"pseudo burins." There are three identifiable burins, and possibly one other; these four have definite negative bulbs of percussion, and were formed on small flakes by two blows being struck at acute angles to another to form a working edge. The "pseudo burins" are of the same general shape, but most of them could easily have been struck off by natural causes.

The amorphous flake and pebble tools seem to have been generally utilized as scrapers, knives, or saws. They are of two general types: (1) those with "scalloped" working edges, that is to say, flakes struck alternately off the working edges; (2) those with planing surfaces, and flakes struck off in sequence on the working edge. Those "amorphous" types, if found alone, could easily be classified as Tayacian. Actually, there are 90 of these implements, all averaging about 4 cm. × 6 cm.; this being 39 per cent of the worked flakes. The next most numerous flake tools are the single-

edged flake-blades, 27, or 11.9 per cent of the flake worked assemblage.

The blades lend an Upper Palaeolithic tone to the assemblages, and extend throughout all the Pleistocene levels. Most of the blades, however, are to be found in the upper four gravels. Only 20 out of the total of 117 were found in the levels below the Fourth Gravels, and out of these, 15 showed evidences of utilization or retouching.

With the exception of one backed blade of La Gravette type found in the Red Gravels III, the blades in the lower levels are more prone to be flake-like than those in the upper levels. Could it be that just at the point where we stopped excavating we were on the threshold of a different industry? Or were we already in a different cultural level? At this point of knowledge, neither can be definitely proven, but the suspicious hint is there. The presence of the single backed blade (a very diagnostic type of implement) might indicate this. Actually, in the Second Sands, we uncovered another blade implement, which was retouched in the backed blade tradition. The tool was backed on one edge, and retouched obliquely opposite the bulbous end.

Signs of utilization or retouching are shown on 60.7 per cent of the blades, but only 43 out of 71, or 60.8 per cent of these, had actually been retouched. Of those retouched, the most numerous types are: 15 blades, with one edge marginally retouched; 12 end scrapers on blades; and 7 blades with opposing edges retouched marginally. Also present were 2 notched blades in the Third Gravels, and a spokeshave in the First Gravels; 3 *limaces* or slug-shaped blade types were found in the Third and Fourth Gravels, and in Red Gravels I. A sickle blade fragment was found near the top of the First Gravels, obviously an intruder from the overlying Neolithic levels.

TABLE 3

BLADES AND BLADE TOOLS — HOTU CAVE, MAZANDARAN, IRAN	Primary Blades	Utilized Blades	Retouched Blades (one edge)	Retouched Blades (opposite edges)	End Scrapers	Backed Blade Types	Knotched Blades	"Limaces"	Sickle Blade	Spokeshave	Totals
First Gravels	24	12	5	0	4	0	0	0	0	1	46
Second Gravels	10	3	1	1	2	0	0	0	0	0	17
Second Sands	2	3	2	0	0	1	0	0	0	0	8
Third Gravels	11	3	3	3	3	0	2	1	1	0	27
Fourth Gravels	4	2	1	1	0	0	0	1	1	0	10
Red Gravel I	0	0	1	0	0	0	0	1	1	0	3
Red Gravel II	0	0	0	1	0	0	0	0	0	0	1
Black Soil under Red Gravel II	0	2	0	0	1	0	0	0	0	0	3
Red Gravel III	5	1	2	0	2	1	0	0	0	0	11
Black Soil under Red Gravel III	0	2	0	1	0	0	0	0	0	0	3
Totals	56	28	15	7	12	2	2	3	3	1	129

Total blades = 129
Total utilized blades = 28
Total retouched blades = 45
Number of blades too small to get accurate individual percentages.

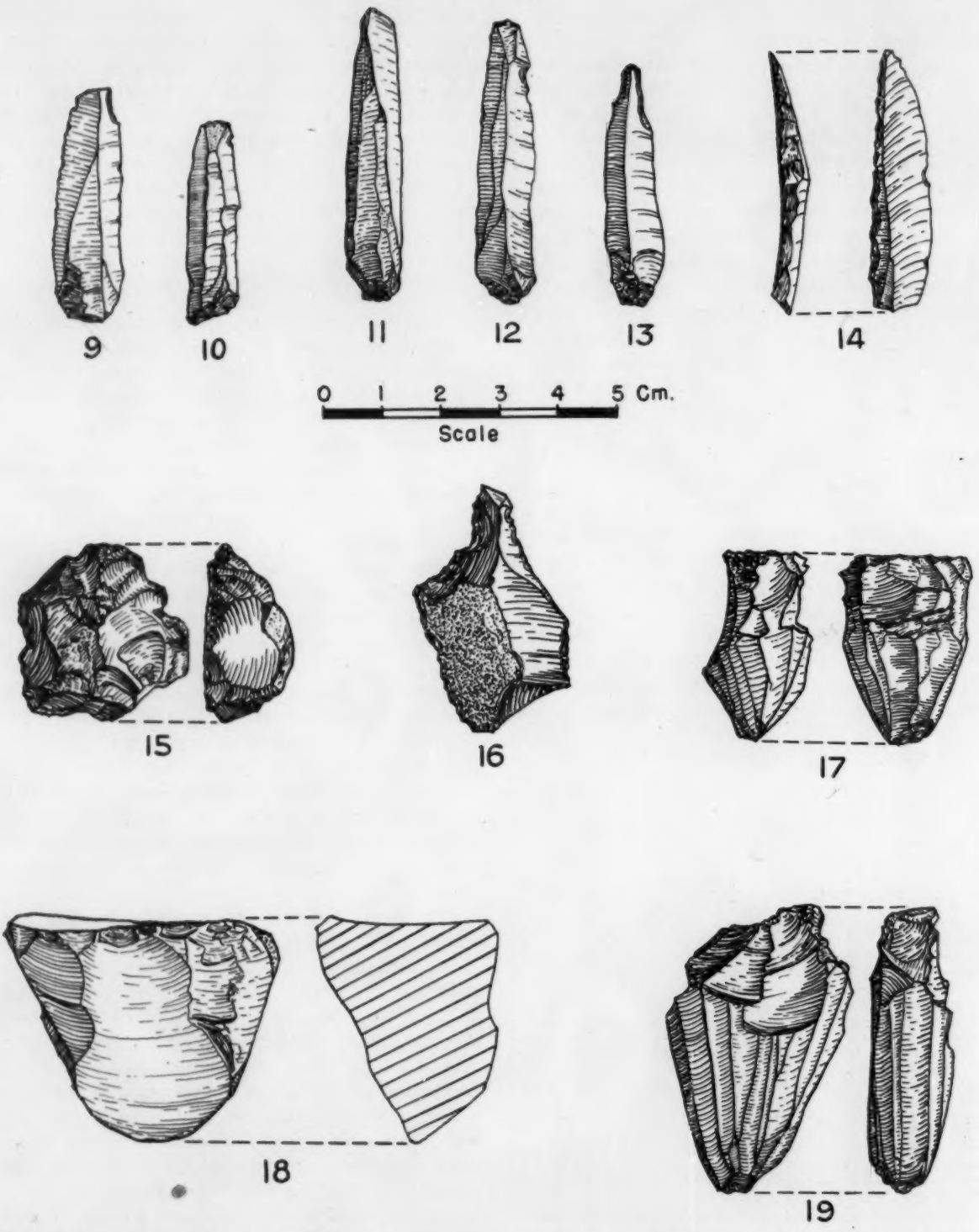
BONE AND MISCELLANEOUS ARTIFACTS

Polished bone artifacts occurred only in the first four gravels, and then they abruptly stopped. None were found in the lower levels. The majority of the bone tools were perforators, and/or needles, chisels, spatulae, and polishers (or *Lissoirs*, as the French would call them).

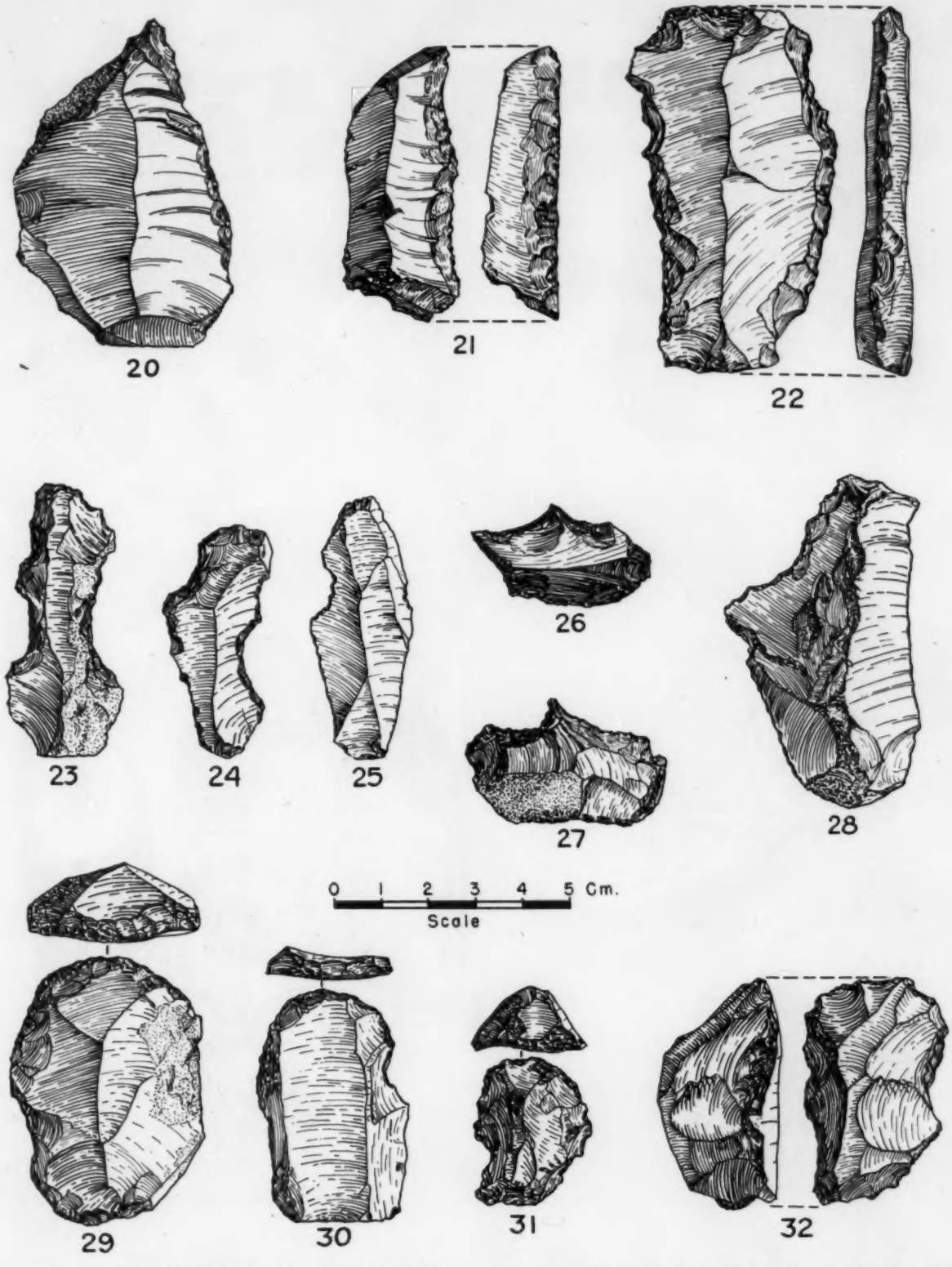
In the Second Sands, and in the Third and Fourth Gravels, a number of broken, hollow bone tubes were found; most of this series were bird long bones, but the largest one was probably *Cervus elaphus*. Dr. Froelich Rainey has suggested that these hollow tubes with beveled ends may have been used for trapping small mammals and birds, in much the same manner as the Eskimo does today. Two slate polishers or "sharpeners," resembling in form the nephritic ones used by the modern Eskimo, were also found in the Third Gravels. A perforated slate pendant (3 cm. × 1 cm.) was found in the Second Gravels.

Three shiny fragments of polished red ochre or hematite were uncovered in the Third Gravels, along with another ochre fragment which had been fired, and had rows of punctations on two opposing sides.

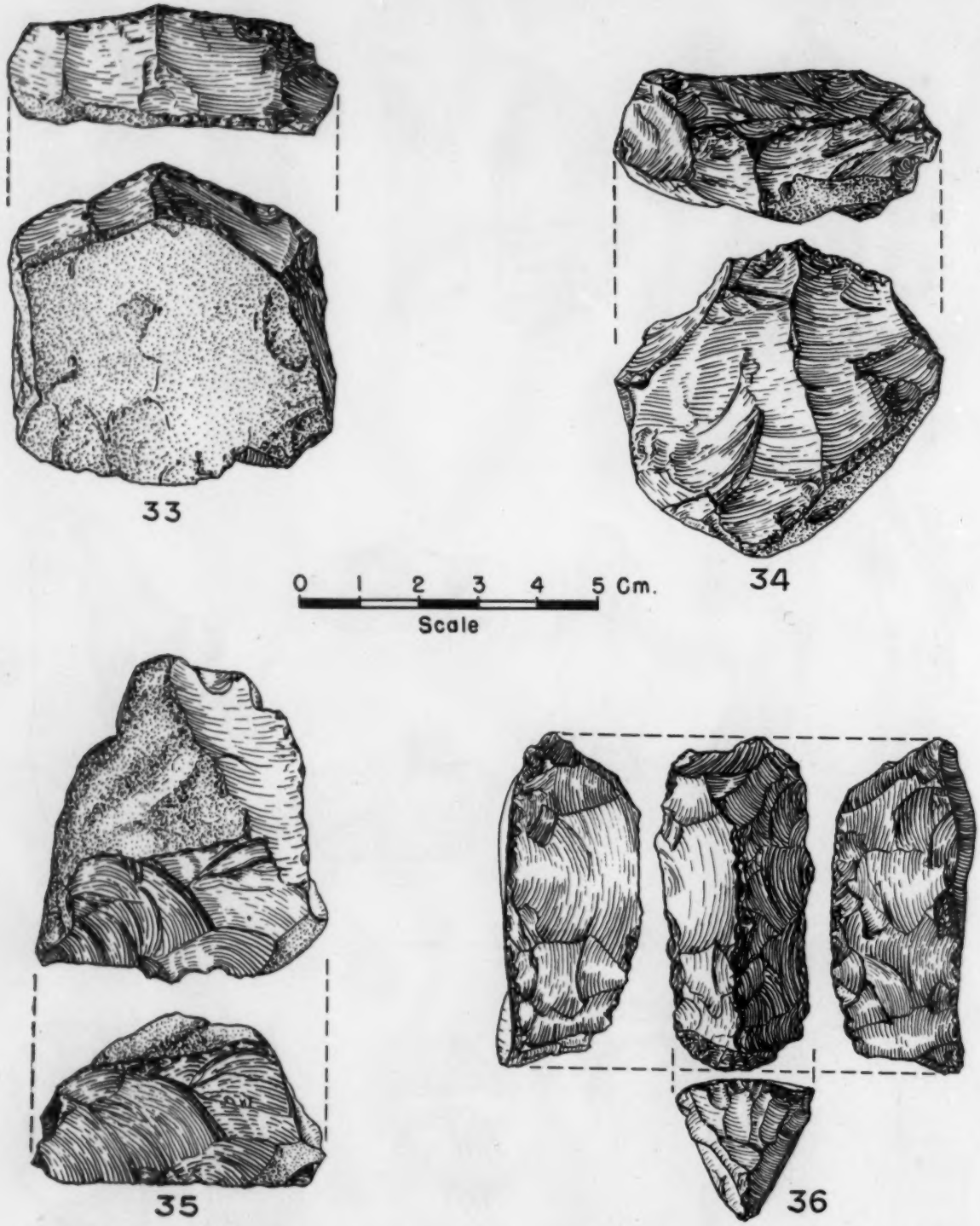
The Third Gravels also produced another item of no small importance—a clay object very tentatively interpreted as being a fragmentary "Female Goddess Figurine." Here are the descriptive facts: 3.4 cm. high by 3.4 cm. wide; crudely fired clay; one side of front broken off; on the unbroken section of the front, there are indications that a pendulant breast had been appliqued in the place where a breast should be, and later broken off; the buttocks are steatopygous; pregnant belly; head and parts of arms missing. The nearest parallels to this "figurine"—if it truly be one—are to be found in the European Russian Upper Paleolithic sites, i.e., Gargarino, Kostienki II, and Epimenko—however, in all these sites, the figurines are made of ivory, bone, or stone, but never of fired clay.



FIGS. 9–12. Primary blades. 13. Perforator. 14. Backed blade. 15. Core scraper. 16. Oblique end scraper on a flake. 17. Micro-core. 18. Keeled scraper. 19. Micro-core.



FIGS. 20–21. Flake-blade, right edge retouched (side scrapers). 22. Flake-blade, double edged. 23–25. Knotched scrapers on blades. 26–27. Borers or drills ("half-moon shape"). 28. Oblique end scraper on a flake. 29–30. End scrapers. 31–32. Core scrapers.



FIGS. 33–35. Amorphous flake tools (scrapers?). 36. Multi-edged core scraper, with five cutting edges.

And yet another peculiar item, this time from the Fourth Gravels: a fire-burned lump of clay was found near a hearth, and this lump of clay shows traces of what may be matmarkings. (See Coon's fig. 23.) Could this indicate the presence of basketry?

SUMMARY AND TENTATIVE CONCLUSIONS

We have at Hotu a flint industry dominated by an amorphous series of flakes and pebble tools, with smatterings of Upper Palaeolithic types; almost all of the tools of both types are manufactured from the locally found, uncooperative, impure cherty-flint, which, incidentally, may well explain the lack of a well-developed blade industry in the Pleistocene levels. There is also a glaring absence of microliths, in spite of the presence of 5 micro-cores; and the one backed blade of true Gravettian type is 3 cm. longer than the longest microlith found in Belt Cave, and it is broken! Belt Cave is actually just around the corner, less than 200 yards from Hotu, and it contains two extensive Mesolithic levels. The bone assemblage and the various items in the artists' kits, such as ochre, could easily be pigeon-holed into any respectable Upper Palaeolithic complex; two points to the contrawise: (1) the presence of the hollow bone tubes and (2) the absence of bone points.

Of course, the dilemma increases when we remember the fired-clay, "female goddess" figurine and the mat-marked fire-burned clay.

Instead of concrete conclusions, the artifacts present a number of concrete problems, which must be solved in the following manner:

1. The excavation of Hotu Cave should be completed all the way to bed rock, and the other known caves in the area should be excavated for further sequential evidence. There are at least six caves and rock shelters in the area around Behshahr actually within one day's walking distance of Hotu.

2. The geological relationships of the cave to the local sequence, to the North Russian sequence, and to the world wide Pleistocene sequence should be studied concurrently.

3. For the present, however, before another group takes to the field, an intensive study of the Hotu assemblages and modes of manufacture should be completed. This would mean studying all the available Middle and Upper Palaeolithic sites in the Middle East, and it would include a study of the Belt Mesolithic material, for although there are only a few specific likenesses to the Belt industries, there may be less obvious similarities in technique which will indicate some sort of genetic relationship with Hotu.

The following points are very important in tentatively assigning the Pleistocene artifacts from Hotu to a new typological niche in Prehistoric Archaeology:

1. We must face the possibility that our brief excursion into the reddish sands and gravels below the first four gravels *may* indicate a new cultural horizon, owing to the fact that (*a*) only 6 of the dominant amorphous flake and pebble tools appear in the lower levels; (*b*) no bone tools were present in the lower levels; (*c*) the only true Gravettian Blade appeared deep in Red Gravel III.

2. This is accentuated by the fact that no ochre, fire-burned clay objects, or slate polishers were found below the Fourth Gravels. Admittedly, these cultural omissions may be due to the incompleteness of the excavations in the gravels, but we must remember that a test pit over two meters deep was sunk below the first four gravels into the lower levels.

3. The absence of microliths similar to those in the Mesolithic levels of Belt Cave.

4. Stratigraphically Belt Cave contains no equivalents to the gravels of Hotu.

5. A classic Upper Palaeolithic Culture should include a well-defined series of blades and blade tools. Hotu, in the Pleistocene levels, has none worthy of the name; and the dominating tool types are the amorphous flake and pebble tools. Paucity of blades is partially explained by the inferior quality of the local flint—as has been previously emphasized.

In view of the above suggestions, we may call the Pleistocene artifacts of Hotu Cave a Localized Upper Palaeolithic (?), but with no clear cut indications as to time or space relationships as yet.[1] This will depend on further research.

It is Dr. Coon's suggestion, and I agree, that, for the sake of symbolic simplicity, we call the culture Mazandaranian. We reserve the right, however, later to partition the culture into such subdivisions as further facts may make necessary.

[1] Drs. Kenneth Oakley and H. L. Movius, Jr., favor a relatively late, even Mesolithic, date for this culture on the basis of typological analogy.

THE HUMAN SKELETAL REMAINS FROM HOTU CAVE, IRAN

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Associate Professor of Anatomy and Physical Anthropology, Jefferson Medical College

(*Read November 8, 1951*)

THREE skeletons plus scraps of two other skulls excavated by Professor Carleton S. Coon of the University Museum last spring from the 4th glacial gravel layer about 30 feet below the present surface of Hotu Cave are a big haul for their Upper Palaeolithic date. But they are not a large enough sample for certainty that they resembled their hunting cousins in Upper Palaeolithic Europe (Morant, 1926, 1929; von Bonin, 1935: sample size about 40) more closely than their possible descendants in northern Iran, the farming and copper-using villagers of 4th millennium B.C. Sialk and Hasan (Vallois, 1939, and Morant, 1938: about 27 skulls available), whose connections will be further clarified when study of the Mesolithic (Coon, 1951) and Neolithic skeletons from Belt Cave can be completed. But the Hotu hunters were certainly large-brained and fully developed representatives of *Homo sapiens*. They show no visible Neanderthal traits. Except for strongly worn teeth and certain functional details of the skeleton they could be duplicated individually today here or in the Near East.

Reconstruction of the remains is still in process, with considerable work still to be done on the less complete crania, numbers 1 and 3. But with the ingenious help of Mr. Albert Jehle, of the University Museum, it has been possible to complete the skull of number 2, to take endocranial and external casts of it, and also to restore the entire pelvis and vertebral column, hands, and long bones of this same person. The burial together of numbers 2 and 3 complicated the whole process of mending the skeletons. It is easy now to separate the complete bones of these two skeletons by their differences in morphology and in color or patination. But the original sorting out of small fragments took more than three times the time needed for a single skeleton (each long bone fragment had to be tried in four possible limbs, each axial skeleton fragment in two possible places).

Dr. Coon has honored me deeply by giving me the task of describing these remains, and I am further in his debt for many keen suggestions both as to restoration and interpretation. I wish to thank Dr. W. M. Krogman and his colleague Dr. A. C. Henriques of the Philadelphia Center for Research in Child Growth for carefully oriented x-ray photographs of skull and teeth, and to thank my colleague Dr. J. E. Healey of the Baugh Institute of Anatomy of the Jefferson Medical College for excellent radiographs of the skull and postcranial skeletons. Most of the daylight photographs of the remains (all the good ones) were taken by Mr. Reuben Goldberg of the University Museum, who also assisted with assembly of the illustrations. Miss Jane Goodale helped both with the original assembly of long bones and with coordinating the records as they progressed. Suggestions of Dr. O. V. Batson, Dr. K. S. Chouke, Dr. C. W. Goff, Dr. W. M. Krogman, and Dr. W. L. Straus are gratefully acknowledged.

Numbers 2 and 3 are the earlier skeletons, found lying partly supine with heads together and lower than their footless legs at the beginning of the 4th gravels just above the underlying sandy layer. The position suggests casual tumbling backwards and down from a higher level close to the cave wall and does not negate the possibility of accidental death through fall of rock disturbed by these or other food-gatherers. Any possibility that the corpses could have been buried from a higher level is contradicted by the fact that the thighs of number 2 had been disarticulated (without shifting the shins or pelvis) and moved over among the bones of number 3. This could not have happened if the bodies had been covered by any great amount of gravel. It suggests that the bodies largely decomposed while lightly covered though weighted with a few rocks, that men or animals shifted the thighbones (and the lower shins with feet ?), and that the ensuing burial was through natural deposition of glacial gravels before decomposition was complete. Number 1, on the other hand, was a deliberate secondary burial placed as a bundle at the top of the 4th gravel layer, with some other skull fragments lying nearby. The unusually good preservation

of the bony tissue is probably a result of their remaining continuously wet down to the time of excavation: any extreme alternation of drying and wetting would have leached away the bones, which were in any case both soft and broken.

Though probably separated in time by a number of generations all the Hotu remains can be treated as one population sample, in the same sense that Morant (1929) and von Bonin (1935) have grouped Upper Palaeolithic Europeans. In both cases identification of actual hunting bands has not been possible except to the extent that as also at Hotu single cave groups may represent breeding units tied to one locality for many generations.

Number 2 was a person about 5′ 6″ tall (167.4 cm. stature reconstruction) with a muscular but slender build indicated by robust bony ridges plus a femoral robusticity index of 11.9 showing a bone with slender shaft. A sharp lumbar curve (index 94.3) combines with a tilted sacrum (33 degrees for the upper 5 vertebrae compared to modern norm of 46 degrees). But the femoral necks are also directed forward (29 degrees) more than usual (13 degrees), a feature unexpected with a pelvic brim tilt as near vertical as 68 degrees (57 degrees norm). Though the depth of the pelvis would tend to place the acetabula in front of the line of gravity, the extra degree of femoral neck torsion should be linked with postural dynamics rather than statics. The upper surfaces of the tibiae are tilted more than usual and the laterally compressed shafts of the shinbones have a diamond-shape cross section. Fibulae are deeply fluted. The femora are distinctly platymeric or thickened transversely in the upper shaft as if to take stress from strong abductor and lateral rotator muscles. The deep gluteal fossae adjacent to marked crests, the strong adductor tubercles, the stressed origin areas for gastrocnemius, and on the tibiae the increased origin area for deep muscles supporting the arches of the feet confirm the suggestion that muscles involved in rough-country travel were well-developed. The hands are long and narrow (*ca.* 187 × 69?), the shoulders probably not broad (clavicle 142? and scapula breadth 94), and the hips narrow (256 bi-iliac). The presacral vertebrate measure about 570 mm., including estimates for intervertebral disks, with an additional 138 anterior arc for the 6-vertebrae sacrum. Though these are not far from lanky modern male dimensions and the skull makes a male first impression the pelvis is clearly female,

FIG. 1. Hotu No. 2, pelvis, lateral radiograph.

with deep "anthropoid" inlet (area 113.1) and wide open outlet (area 106.3, interspinous breadth 104?, and sub-pubic angle 88 degrees). The sacro-sciatic notches are large (37 × 61 with posterior segment 23 by Letterman's technique). Pubic and ischial lengths of 77 and 86, measured externally (Washburn, 1948), are just on the borderline of the female category for the white race. But the extremely deep and sharp-bordered pre-auricular sulci and the roughened ligament and rectus abdominis and aponeurosis attachments on the symphysial area of pubic bones suggest stress, perhaps from carrying children. Pubic symphysis is early Phase V.

Thus this woman of about twenty-seven years was a real Amazon, stronger, taller, and slenderer than average of ancient Greek, Medieval Norwegian, or modern American women.

The pentagonoid skull vault is 75 to 100 cc. larger than the 1,325 cc. modern average in capacity, and is long and high with marked post-coronal depression and concave and sinuous temporal planes, as if the infantile sharp curve of the parietal bone had not been fully corrected by later peripheral remodelling. The cerebellar area bulges downward, and the endocranial cast stresses this vertical development and a somewhat ill-

FIG. 2. Hotu No. 2, supero-medial aspect of left maxilla.

filled appearance. These linear vault features are typical of peoples found across the desert zone of Africa and the Near East. But the broad skull base (bi-auricular 116), the relatively wide and squat face (height 105 without allowance for teeth wear), the notably low, rectangular, but drooping (10 degrees) orbits, the small nose, and the almost horizontal chewing plane (2 degrees) combined with square jaw angles and marked protrusion of chin are Cro-Magnon-like and quite unlike the deep, convex, beaky, sloping-jawed, and narrow later Iranian face. Molar teeth are not taurodont.

Number 3 is the skeleton of a small, stocky, overworked woman, about thirty-seven (pubic symphysis Phase VII), and about 5′ 2″ tall (156.9 cm.). She has relatively thick and bowed femora, and extra stress on the intertrochanteric line and insertion of the ilio-tibial band on the tibiae. Otherwise she shows the same features of muscle dynamics as number 2. The olecranon fossa of the humerus is perforated, forearm bones are bowed and wider set than number 2's, the hands are stubby (*ca.* 176 × 78), and the pelvis probably small and broad. The bones seem slightly thinner in cortex and less heavily trabeculated than those of number 2, and hypertrophic arthritis appears in the lumbar vertebral bodies, pelvic joints, and hands. Both greater multangular-metacarpal joints show marked exostoses and eburnation worn by excessive use of thumbs for motions of opposition rather than extension-flexion. And the fifth left metacarpal distal shaft was fractured with little displacement in healing but marked flanging of ligament attachments.

The skull has a strikingly capacious vault (1420–1460 cc.), ovoid, broad, well-filled with wide-set base (129 mm.) and approaching the "square-head" minority among Upper Palaeolithic and later Europeans. The face has wide cheeks, wide nose, and a protruding chin, and probably resembled number 2.

Number 1 is the fragmentary skeleton of a massive male, between thirty and forty years old, very muscular and tall: over 5′ 9″ (175.7 cm. based on radius length of 261). In body build he was probably the male match of number 2. The skull has strong and sharply cut browridges, sloping forehead, flat temporal region, arched nose profile, and exceedingly heavy mouth (palate 59 × 64, chin 35) with the usual prominent Hotu chin and square-profiled jaw angles. The lower tooth row is wider than the upper. It is possible that this skull fits directly between the modes of Upper Palaeolithic Europe and Chalcolithic Iran.

Number 4 is a left maxillary fragment of a female (?) of about fifteen or sixteen, probably with a wide palate and wider nose and more prognathism than number 2.

Number 5 is a vault fragment perhaps like number 3.

Thus the Hotu sample does show sharp variation, probably equalling the heterogeneity found either among European Upper Palaeolithic hunters or metal age Iranians as wholes. Morant finds the former less variable than seventeenth century inhabitants of London (1929: 135) in spite of the slight split between linear and lateral skull form tendencies (*cf.* von Bonin, 1935). And we need not expect to find in either Palaeolithic or Chalcolithic North Iranians the degree of heterogeneity seen in the Bronze Age and later in the Aegean area (Angel, 1951). But a sharp contrast is suggested at Hotu between a desert body built as seen in number 2 and a temperate or cold climate stocky build as seen in number 3. This contrast between linearity and large surface relative to mass in desert environments and the opposite in cold climates though proven only for animals

(Bergmann's and Allen's rules examined by Rensch, 1936) fits roughly the body build differences seen in maps 5, 6, and 7 of Coon (1939: 251–279): Europeans as a whole are more massive and less dolichocephalic than inhabitants of the whole desert strip south of the Mediterranean. Coon, Garn, and Birdsell (1950: 36–45) have greatly extended this theory beyond the suggestions of Geyer (1932). And I have tried to show (1949: 443) the probability that Upper Palaeolithic Europeans were actually very massive people as suggested in exaggerated form by various figurines (Burkitt, 1939: 162–166; Martin, 1928: 237–241). The experiments of Ogle (1934) and of MacArthur and Chiasson (1945) show how both the direct effect of climate and strong selection can produce through heterogony marked changes in adult body build. But where, as at Hotu, we are dealing with a single population we may assume that body build *differences* are more likely to be genetic than environmental in origin.

On the other hand such features as the Hotu mouth and chin combination, the Cro-Magnon-like low orbits, the Iranian full cerebellum, and perhaps trends toward sacral hiatus and first molar abscess point to a certain genetic unity, and plausibly "inbreeding." One would expect a small and inbred population to show just such discontinuities and uniformities in contrast to the "blended" variation of a large pan-mixed population. This situation plus the double resemblances of the Hotu skeletons to massive Cro-Magnons and to linear Iranians (*cf.* von Bonin, 1935 and Morant, 1926, 1929 *vs.* Vallois, 1939 and Morant, 1938) shows the kind of evolutionary plasticity expected at this date in a peri-glacial locality. There is even a possible resemblance to the old man and the large woman (Nos. 101 and 102) from the late Upper Palaeolithic at Chou Kou Tien (Weidenreich, 1939). The changes and connections implied by these comparisons would be a necessary result of genetic drift plus mixture plus climatic selection in a more or less continuous chain of occasionally mixing small breeding groups. The theories worked out and tested by Wright (1931, 1948), Dobzhansky (1941, 1947), Huxley (1942: 40–44), and others clearly apply here. I hope that the Hotu data and comparisons in the light of these theories of population change deal the *coup de grace* to the outmoded hypothesis of racial origins which sought sources for later racial types (often imaginary) from small, "pure" inbred groups in the distant past. These sources just do not exist.

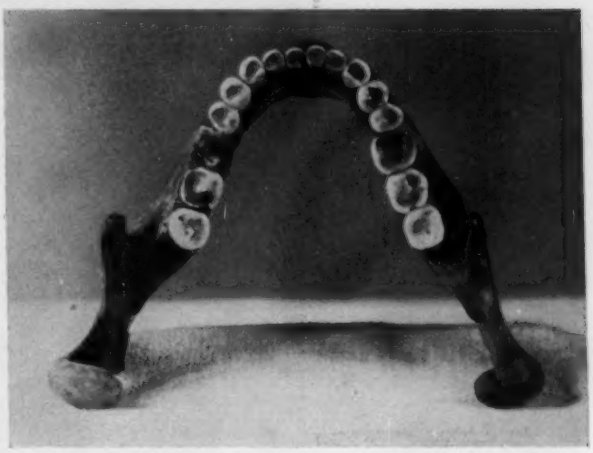

FIG. 3. Hotu No. 2, top view of lower jaw.

In this cursory analysis I am using functional units like the constricted skull vault of the desert zone or the unstressed nasalia seen where palate is horizontal and chewing thrust almost vertical in the hope that their genetic determinants may turn out to be simple. We could then eliminate the present contradictory confusion of arbitrary measurements, isolated details of form, and vague types. To make any list of such units, however, we need not only a deeper knowledge of human genetics but also to know better how far one mechanically dominant region of the body affects adjacent regions and how plastic the body is in adapting to all kinds of stress during growth. In the present instance most of the multiple Iranian versus Cro-Magnon facial contrast seems to depend directly on vertical growth at nose-forehead junction versus jaw condyle, in relation to the anterior skull base: in Hotu number 2 both the anterior skull base and the occlusal plane come closer than usual to paralleling the Frankfort plane whereas in the later Iranians, with their buttressing noses, the chewing plane, and probably also the anterior skull base, slope forward sharply away from the Frankfort plane as also in Neanderthal man and the Eskimo. The Hotu-people seem to have been adequately nourished (tall stature and deep pelvis), but have flattened shafts of long bones and extra ridges to increase surface for muscle origins, as seen in almost all prehistoric and many primitive peoples. To test how much the facial pattern is genetic and will respond to genic loss and selection and how far

FIG. 4. Hotu No. 2, right profile, front, and back views of skull.

FIG. 5. Hotu No. 2, left profile, basal, and top views of skull.

FIG. 6. Hotu No. 3, left profile, back and top views of skull before face restoration.

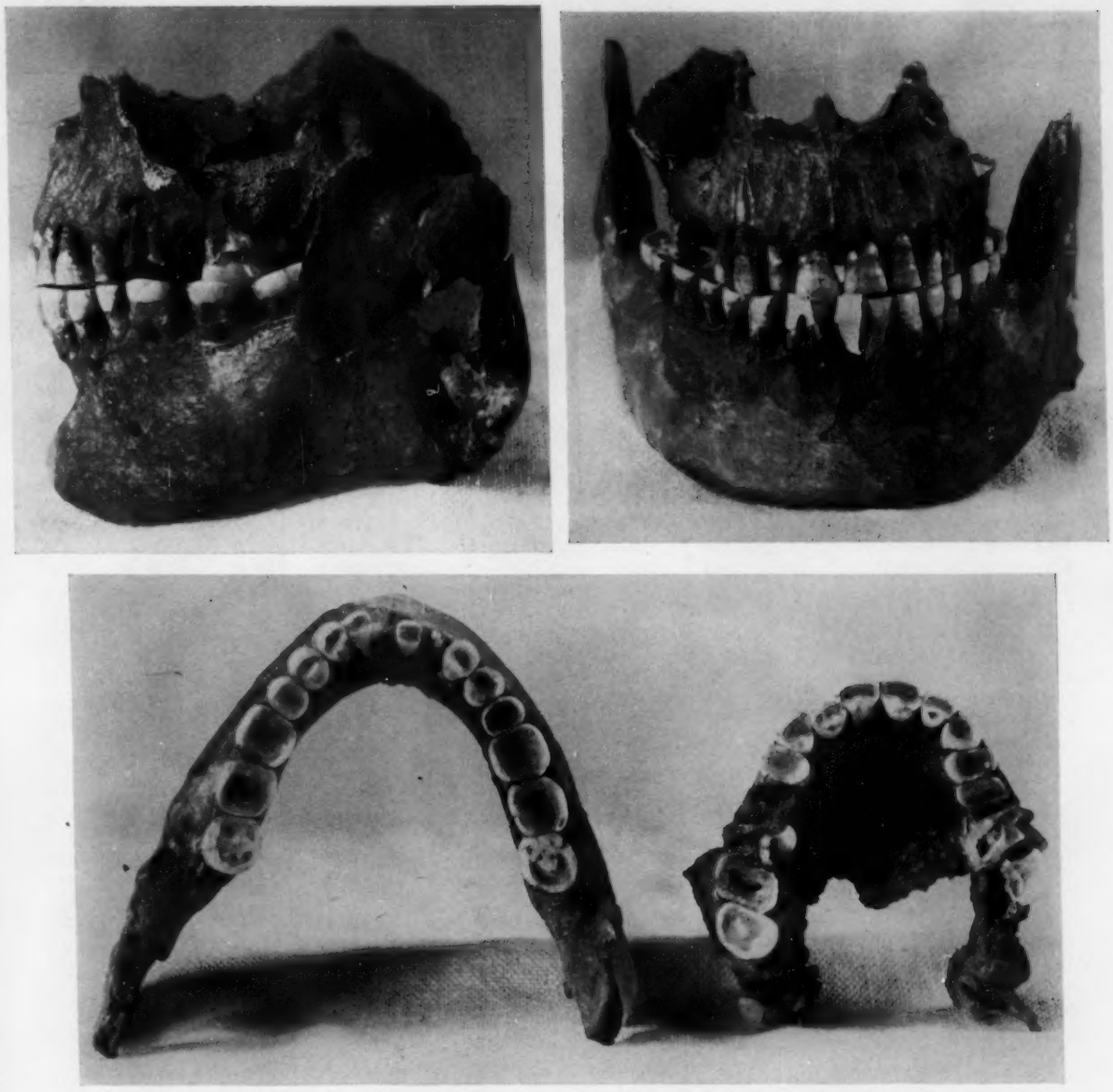

FIG. 7. Hotu No. 1, upper and lower jaws with teeth, before further restoration.

muscle use is environmental and only weakly responsive to selection demands animal experiments.

Further speculations in the social biology of the Hotu group are interesting. All three youngish adults described have suffered from peri-apical abscesses of the upper first molar alveolae extensive enough to invade the maxillary sinus. This is clearly seen on the left side of the palate of number 2, in whom a secondary drainage pathway opened in the canine fossa 11 mm. below the infraorbital foramen after the wall of the maxillary sinus had been thickened to 1.5 mm. by the inflamed mucoperiosteum. Though some inherited factor affecting dentine reaction may be partly responsible, this abscess formation is chiefly a result of excessive teeth wear, amounting to about 1 mm. every five years in the molar region of number 2. Dietary conditions must have been truly eskimoid to produce such abscesses mechanically rather than through tooth decay.

Femoral neck torsion, tibial head tilting, gluteal

TABLE 1

MEASUREMENTS OF HOTU FEMALE SKULLS COMPARED WITH MEANS OF OTHER GROUPS

Area	Iran				Europe			
Culture period	Upper Palaeolithic		Chalcolithic and Copper Age		Upper Palaeolithic		Medieval	
Site	Hotu		Sialk, Hasan		West and Central		Oslo, Norway	
Authorities	Coon and Angel		Vallois, Morant		V. Bonin, Morant		Schreiner	
	No. 2	No. 3	M	N	M	N	M	N
Cranial capacity (seed)	1,404 cc.	1,440?	1,293	(calc.)	1,374	(calc.)	1,302.6	326
Horizontal circumference	505	520	502.2	8	524.6	11	506.3	538
Transverse arc	309	309	296.0	3	306.2	7	301.0	495
Sagittal arc (total)	382	388	372.3	6	372.9	10	360.0	536
Frontal arc	127	135	125.5	4	125.2	13	123.8	561
Parietal arc	137	129	129.9	4	127.7	14	121.9	567
Occipital arc	118	114	116.3	3	121.0	10	114.6	544
Cranial length (gl.-occ.)	186	184	179.7	12	185.6	12	179.3	572
Base length (bas.-nas.)	(104)	(95)	95.5	3	100.1	9	96.5	479
Basion-bregma height	(139)	(138)	129.2	3	132.4	9	126.2	489
Auricular-vertex ht.	120	120−	111.5+	6	112.1	5	108.9+	500
Maximum vault breadth	135	143	132.7	12	138.2	13	135.8	555
Base breadth (bi-auric.)	116+	128+	(narrow)		(wide)		119.0	496
Bicondylar breadth jaw	116?	123??	113.0	3			113.0	109
Maximum frontal breadth	116	118	113.0	10	117.6	13	114.8	434
Minimum frontal breadth	93+	97	92.6	10	97.3	13	93.5	546
Face breadth (bizygomatic)	129?	138??	122.7	5	129.4	5	124.5	284
Jowl breadth (bigonial)	99+?	(99)	94.2	4			94.6	97
Face height (total)	105	(103)	116.5	2			110.7	109
Chin height	34	33??	(34.5)	2	(34 ca.)		30.2	101
Upper face ht. (na.-pr.)	65	61?	66.5	5	65.9	7	67.2	325
Nose height	46−	44.5?	49.0	4	48.5	8	48.8	335
Nose breadth	21+	26?	24.3	4	25.6	10	23.5	311
Interorbital breadth	20	(23)	18.0	2			20.6	326
Orbit breadth (dacryon)	38	40?	39.5	5	(40.5)	8	(38.4)	345
Orbit height	28	31+?	32.6	5	29.9	10	33.4	346
Palate length (external)	56	53−?					50.2	254
Palate breadth (external)	62	63?					60.3	248
Ramus height jaw	65?	(65)					58.4	113
Least ramus breadth	35	30					31.6	113
Jaw angle	110	(112)	110.7	4			121.3	111
Facial profile angle	88	88?	84.5	4	84.8	7	84.4	281
Nasal bones profile	(65)						56.2	211
Cranial l-br. index	72.6	77.7	74.0	12	74.9	11	75.8	553
Mean auric. ht. index	74.8	73.9	71.4+	6	69.2+	*	69.1+	*
Frontal-parietal br.	68.9	67.8	70.2	10	70.3	12	69.0	531
Cranio-facial br. index	95.6?	96.5??	90.1	5	93.6	*	91.7	276
Upper face ht.-br. index	50.4?	44.2?	53.3	4	50.9	*	54.1	262
Nasal ht.-br. index	45.7	58.4?	49.9	4	53.3	7	48.1	302
Orbital index (dacryon)	73.7	77.5?	82.4	5	(73.8)	*	(87.0)	338

Note: Presumably 1 mm. should be added to the auricular height (OH) of Chalcolithic Iranians and almost 1.5 mm. added to the auricular-bregma heights of the European series to equate the porion-vertex dimension used for Hotu. An attempt has been made to correct the European series orbital breadths to equate *dacryon*-ectoconchion. Figures in parentheses are estimates and not reliable for statistical purposes. 5 mm. have probably been lost from total facial height through incisor teeth wear of Hotu No. 2. and No. 3: the amount of compensatory alveolar growth is not determinable.

TABLE 2

MEASUREMENTS OF HOTU FEMALE SKELETONS COMPARED WITH MEANS OF OTHER GROUPS

Site	Hotu		U.S.A. Modern		U. Pal. Europe		Norway Med.	
	No. 2	No. 3	M	N	M	N	M	N
Humerus, Max. length	(315)	(297?)	320.3	100			310.5	312
Max. midshaft thickness	22	21					20.9	312
Min. midshaft thickness	16	14					16.2	312
Bi-epicondylar br.	53–	58						
Radius, Max. length	238	208	217.8	100			233.1	171
Ulna, Max. length	260	233					251.2	133
Forearm max. breadth	42	(44)						
Femur, Max. length	461	396–	422.4	100				
Bicondylar length	455	391+	417.6				417.4	493
Max. diam. head	43.5	40.5					42.6	415
Subtrochanter A-post.	25.5	22.5	23.9	28			24.0	497
Subtrochanter Transv.	31	28.5	29.4	28			29.2	498
Midshaft A-post.	27.5	26	26.4	28			25.6	499
Midshaft Transv.	26.5	24	25.8	28			25.1	499
Distal epiphysis br.	74	73						
Neck torsion angle	29	31					12.7	437
Tibia, L.c. length	(358?)	(305?)	338.9	100			333.1	544
Nutr. for. level A-post.	32.5	33.5						
Nutr. for. level Transv.	21	21						
Retroversion angle	10.5	13.5					7.4	379
Pelvis								
Innominate height	207	189	202.0	20				
Innominate breadth	154	(136)	157.3	20				
Bi-iliac breadth	256		270.5	20				
Inlet of true pelvis A-post.	121		122.4	500				
Inlet of true pelvis Transv.	119		130.6	500				
Outlet of true pelvis Ant.-post.	109, 120 at S5		119.7	500				
Outlet of true pelvis Intertuberous	125		109.0	500				
Outlet of true pelvis Interspinous	104?		99.5	500				
Pubic length	77?	(82)	77.9	100				
Ischial length	86?	79?	78.3	100				
Sciatic notch breadth	61	55	51.9	106				
Sciatic notch height	37	33	30.9	106				
Sciatic notch post. segt.	23	24=	27.6	106				
Pelvic brim tilt	68		57.7	500				
Sacral height	118, 108 for 5		101.8	25				
Sacral breadth	(111)		117.5	25				
Lumbar Vertebrae Hts.								
Anterior heights	139?							
Posterior heights	133?							
Indices					both sexes			
Humeral flatness	72.7	66.7	79.3	770			77.7	312
Platymeric (femur)	80.6	79.0	81.1	28	74.5	16	82.2	498
Pilastric (femur)	103.8	108.3	101.7	28	116.0	8	102.1	499
Robusticity (femur)	11.9	12.7	12.5 ca.				12.2	*
Cnemic (tibia)	64.6	62.7	(72) ca.		64.6	16		
Pelvic inlet	101.7		93.9	500				
Vertical lumbar (curve)	95.7?		96.2	43				
				males				
Stature reconstructed					females			
(Dupertuis' general formula)	167.4 cm.	156.9	161.0	100	155 ca.	5	161.0	*

Note: Authorities for the composite series of modern white female data include: Dupertuis and Hadden, Hrdlička, Young and Ince, Letterman, Washburn, Todd and Pyle; Von Bonin records the Upper Palaeolithic indices, and Wagner the Norwegians.

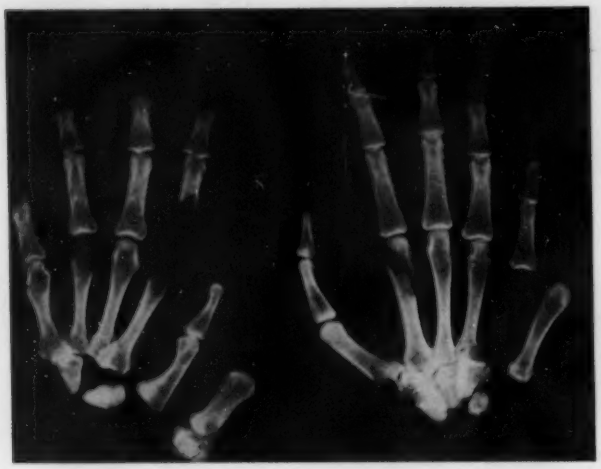

FIG. 8. Radiographs of hands. Hotu No. 3 (*left*) with right thumb added; Hotu No. 2 (*right*) with left fifth metacarpal substituted. Note pathologic changes in No. 3 at head of fifth metacarpal and first metacarpal-greater multangular joint.

crest development, platymeria, platycnemia, and stressed extensor and rotator muscle insertions form a complex (*cf.* Wagner, 1926: 115–117) called the bent-knee gait, often misinterpreted. This applies to use of the legs flexibly, like a skier, and *not* a posture. Stress on the ilio-tibial band, iliac crest, and lower and upper lumbar areas (possible herniation of lowest nucleus pulposus in number 2) suggests further that the Hotu women may have done some standing and working with braced legs (as pulling on a fish net) as well as much climbing in rough country, carrying, and digging. The injuries to the thumb-wrist joints and little finger of number 3 suggest possible fighting but more plausibly hard manual work perhaps more specialized than digging for roots: flint-chipping, plaiting baskets, net-making, or possibly midwifery or shamanism. The pelves of the two women show enough bone reaction at ligament attachments and insertion of the abdominal wall muscles (rectus and external oblique aponeucosis) to hint that pregnancy may have been frequent and without rest period. No lines of arrested bone growth occur, though one might imagine the lack of parietal remodelling and a trace of occipital pitting in number 2 as evidences of a barely adequate diet. Perhaps it was a diet more than adequate *in toto* but undergoing marked fluctuations with the game supply. At any rate life expectancy may not have been less than in Neolithic times in spite of these physical and social pressures. But life was certainly hazardous, if one follows the indication that the two Hotu women were killed by a rock fall.

From the social standpoint this gives a picture of a small band of gatherers with little leisure, where every one worked at top capacity, where children were treated well, fed as regularly as possible, and valued, where strangers might be admitted, where physical exuberance was admired, and where accident rather than disease might cause death.

From the biological standpoint the Hotu sample places modern man definitely on the edge of the steppe zone as well as in the caves and ambushes of Europe, of China, and of North America during the retreat of the last glaciation. This raises three questions: "How wide, how rapid, and how disastrous were migrations of our hunting ancestors? How important was individual initiative in such groups? How old and how selected is modern man?"

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AIRPLANE OBSERVATIONS OF HOMING PIGEONS

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INTRODUCTION

By what mechanism does a bird home after being released in unfamiliar territory? Is it by exploration or by navigation? If by exploration, is it of the radial type or spiral type, or just random flight until familiar territory has been reached? If by navigation, to what stimulus or stimuli does the bird respond? The probability that guidance in migrational flights is a manifestation of the same mechanism exhibited in "artificial" homing experiments broadens the significance of the problem.

Wild birds and homing pigeons marked with leg bands or by other means have long been used in studying homing ability. Usually the birds have been released at a distance from the nest or loft and only the direction of flight from the release point and the time required in homing noted. The obvious shortcoming of this method is that usually nothing is known of the route taken by the bird after its disappearance from the release point. To overcome this gap Griffin (1943) pioneered in using the airplane, following herring gulls. Later Griffin and Hock (1949) were successful in following gannets long distances. Griffin (unpublished) demonstrated the feasibility of using the airplane in studying the behavior of homing pigeons. Yeagley (1949) employed the airplane in some of his studies of homing pigeons.

To study the behavior of the homing pigeon, *Columba livia*, many flights were observed during the summer and fall of 1950. Most of the flights were by one- and two-year old birds trained to home to their loft in Middlebury, Vermont. Some flights of Middlebury youngsters (hatched 1950) were also followed, as well as youngsters from the Signal Corps lofts at Fort Monmouth, New Jersey. In all cases flocks rather than individual birds were followed. Some flights were made within presumably familiar territory, others in territory presumed to be unfamiliar to the pigeons.

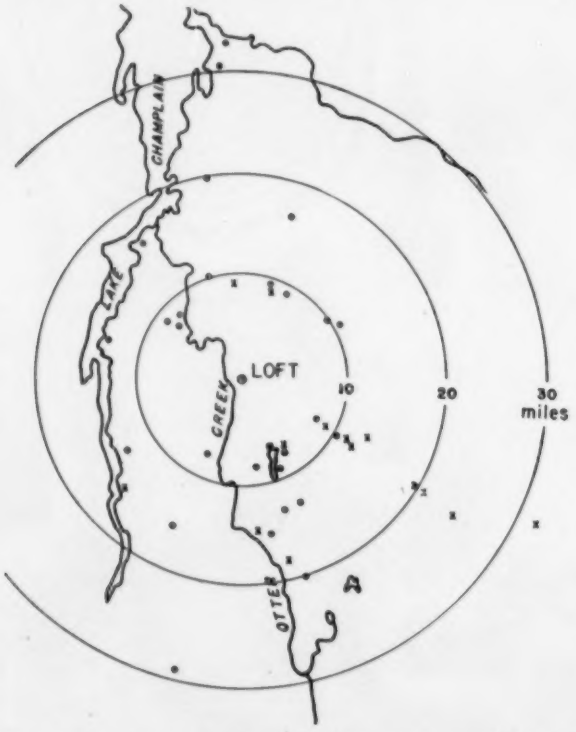

FIG. 1. Release points for Middlebury birds, 1948–1949. O = 1948; X = 1949.

ACKNOWLEDGMENTS

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MIDDLEBURY BIRDS AND TRAINING FLIGHTS

THE FLOCK

The flock was founded in 1948 by youngsters from the Signal Corps lofts at Fort Monmouth. In 1950 more birds were received from Fort Monmouth and one youngster was purchased from a Vermont breeder. Young were raised at the loft in the spring of 1949 and 1950. When flights were begun in 1950 the flock of old birds consisted of 39 birds from previous years, 2 strays from distant lofts that refused to leave when liberated, and 2 new arrivals from Fort Monmouth. The 4 strange birds were soon lost. The rest had had one or two years of training in the Middlebury area. Figure 1 shows the places from which these birds had homed in 1948 and 1949. During the spring birds had their freedom until training began, after which they were out of the loft only on homing trips or for exercise before being fed.

LOFT AND SURROUNDINGS

The loft is of the type used by the Signal Corps (fig. 2), and is located on the eastern edge of the village of Middlebury, Vermont, at the Middlebury College farm. The village lies in the Champlain valley at an altitude of about 400 feet, and 4 miles west of the Green Mountains bordering the valley. Lake Champlain lies 12 miles to the west, here less than a mile wide, and beyond it the Adirondack Mountains, a mass of irregularly arranged peaks with elevations of 4,000–5,000 feet. The Green Mountains show in general a north-south arrangement of wooded ridges with elevations in this region of about 2,000–4,000 feet. Local geographical landmarks include Otter Creek, a stream averaging about 100 feet in width, and Chipman Hill, half a mile north of the loft, rising some 400 feet above the valley floor. Cultural landmarks in addition to the village itself include a conspicuous group of college buildings on a hill a mile west of the loft, highways, and a single track railroad running north and south.

FLIGHTS OF ONE- AND TWO-YEAR OLD BIRDS

Flights began July 1 when the birds were released 4 miles from the loft. The first flight followed by plane was on July 5, from the Bristol Airport, 9 miles from the loft. Thereafter almost all flights except shorter ones for exercise

FIG. 2. The loft at Middlebury. Birds in air are arriving from a homing flight. Photo by *Parade Magazine*.

were followed by plane. During the first part of the season the birds were usually released in two groups, the composition of which was determined by chance. Except on longer flights no record was kept of individual birds. For shorter distances transportation was by car, the crates being placed in the trunk compartment and the top closed except for a few inches to permit circulation of air. Twice shipments were made by railway express; the longest were made by airplane. Releases were made on the ground upon signal from the pilot (fig. 3). The birds were not fed before flights, but were given food upon their return. None had nests or dependent young during the investigation.

PLANE AND OBSERVATION

Most of the observations were made from a Piper Supercruiser, a high-wing monoplane capable of being flown at but slightly greater speed than that of the birds. Its tandem seating arrangement enabled both pilot and observer to watch the birds simultaneously from one side.

FIG. 3. Birds being released at Bristol Airport. Photo by *Parade Magazine*.

The plane was usually flown about 1,000 feet above the birds. At no time did the pigeons give any indication of being influenced by the plane. While flying in the region of New York City the plane's altitude was increased to 2,500–2,800 feet and the birds followed without much difficulty.

On flights in Vermont and New Hampshire the homing routes were marked directly on government topographic sheets (fig. 5). Because of their scale of 1:62,500 (1 inch to the mile) it was possible to indicate with considerable accuracy the position of the birds at all times. On other flights aeronautical charts with a scale of 1:500,000 were used.

Homing flocks frequently broke up into smaller groups. This was particularly true on the longer flights. In such cases our practice was to follow the largest group, if possible. In flights where flock defections occurred, the routes charted in most instances show the course of the majority of the birds. In many instances individual birds undoubtedly homed more directly than the diagrams suggest.

Not all flights were followed to completion. Sometimes the cause for failure was the necessity of banking the plane in such a way as to cut off the birds from sight for a few seconds. On other occasions it was inattenton on the part of both the pilot and observer. Once the birds eluded observation by outclimbing the plane. On several occasions the birds were abandoned after they had landed before reaching the loft. In practice both pilot and observer watched the birds except for brief intervals when one attended to other duties. The birds do not make a conspicuous target. They are difficult to find if once out of sight, and often difficult for one of the observers to point out to the other. In the vicinity of airports, where constant watch had to be kept for other aircraft, the pilot devoted his entire attention to the birds while the observer acted as lookout. Because there were usually birds of several plumages in the flock, little difficulty was experienced because of obliterative backgrounds. Over woods and green fields the dark birds were difficult to see, but over fields of ripe grain they stood out well. Birds with light plumage were conspicuous over green backgrounds but hard to see over some of the lighter backgrounds. On sunny days reflections from water were bothersome, but as few flights were over large bodies of water no flocks were lost from this cause.

BEHAVIOR OF FLOCK UPON RELEASE

Releases were made by opening the door of the crate in whichever direction offered the best takeoff for the birds, without regard to the direction of the loft. Birds transported by plane were carried in paper cartons that opened at the top. Upon release the birds usually rose 200–300 feet and flew in the neighborhood several minutes. Although "circling" has been used by others to identify this preliminary flight, the term "orientation flight" (Hitchcock, 1950) seems preferable, for little of the flying is in circles, but rather in various directions. At first the flight is close to the release point, but sorties of several miles may be made later. When released in hilly country the birds tend to fly to nearby elevations, circling them at intervals during the period of orientation.

When the birds were released in two groups, the first usually spent more time in orientation flight than the second. One might imagine that the first group is reluctant to leave without those still caged. Likewise the second group may be imagined to be eager to rejoin the first, and to be hungrier as well, if the interval between releases has been long. Possibly a certain time interval at the release point is necessary for the birds to recover from the effects of their journey, or to acquire a degree of orientation, as the following experience suggests. On one occasion when the birds were released as a single flock a few minutes after their arrival they showed marked confusion. An orientation flight of 20 minutes ended when the birds perched in a tree, refusing to leave it during the next 45 minutes in spite of attempts to stimulate them to do so by diving the plane close to them. Thereafter the birds were given at least a half-hour's rest before being released.

The exact time when orientation flying ends and the homing flight begins is sometimes hard to determine. Characteristically, however, the homing flight is at a lower altitude, and is continued for a long stretch without significant change in direction. Two flocks timed over a three mile stretch near the loft showed a speed of 45 miles an hour. When attacked by hawks their speed is visibly increased.

THE HOMING FLIGHT: FLOCK FORMATION

Flocks homing on a good course are in the form of a slight arc, the birds flying close to each other, side by side, with the convexity of the line in the direction of flight. Individual birds change position within the flock. Flocks on a bad course are apt to show a looser formation; the arc is wider and individuals or groups on the flanks pull away from the rest. Parting from the flock appears to be a difficult feat, the deserters usually not making the separation final until after several false starts. It is as though an invisible elastic band joining the flock and deserters, after several stretchings, finally snaps.

ALTITUDE

The usual altitude of the birds while homing is at about the level of the treetops. On several occasions they were seen to rise in order to clear rows of trees in open fields. Flocks on a poor course sometimes rise about 1,000 feet above the terrain and fly long distances at that altitude. Major elevations in the path of flight may be anticipated either by straight climbing or by spiraling upward. If flying close to the ground the birds may zig-zag up a long, steep slope. Narrow valleys may be crossed from rim to rim without following the contour.

INFLUENCE OF THE TERRAIN

No consistent tendency to follow geographical or cultural landmarks was observed. The Middlebury birds showed a decided preference, however, for wooded rather than open country. They also preferred ridges and even minor outcroppings to level land. These preferences did not alter significantly the direction of flight. Such tendencies were not observed in the Fort Monmouth birds. In mountainous country the birds did not utilize passes even when but a short distance from their course. On the other hand they skirted the highest peaks. Large bodies of water were crossed reluctantly by inexperienced birds. Small ponds were usually skirted. On several occasions flocks nearing the loft followed a sharp jog in the river. Although this appears to be an instance of following a landmark, it may be but a manifestation of their preference for the wooded river banks rather than the open fields in the direct path.

WEATHER

Flights were made under varied conditions—on sunny days, on days when the sky was completely overcast, on days when the visibility extended to the horizon, and on days when haze cut visibility to a few miles—without apparent effect on the homing performance. A few short showers were encountered, but no prolonged rains. Most flights were made when the velocity of the wind at ground level, as determined by a portable "Windicator" or by regular anemometer at airports, was less than 15 miles per hour. Considerable turbulence in mountainous regions was encountered, both the birds and the plane being buffeted by strong air currents. On two occasions when the birds were not followed by plane fog may have interfered with homing. Young birds released in a fog-free valley across a cloud-shrouded ridge from the loft failed to return the same day, and several were lost. On another occasion all birds, young and old, were released about sundown for exercise less than 3 miles from the loft. The clouds that evening were but a few hundred feet above the ground, though the loft area was visible from the release point. No birds returned until morning. In each of these instances disturbing factors other than fog may have caused the poor homing.

HAWKS AND OTHER DISTRACTIONS

Hawk attacks were frequent. Usually the hawk was seen, but occasionally its presence was inferred from the pigeons' behavior. The flock may disintegrate with explosive suddenness when attacked. Some birds drop, apparently to hide in the trees; some outclimb the hawk, and others race off in all directions. On only one occasion was a hawk known to have caught a bird. The victim was the runt of the flock, clearly the least fit member. Attacks consisted of one or two dives, and usually most of the birds soon regathered. After an attack the birds may fly a few minutes and then perch. Large soaring hawks passed close to flocks without causing any disturbance. Flocks on good course and during orienta-

TABLE 1

FLIGHTS OF ONE- AND TWO-YEAR OLD BIRDS, FOLLOWED BY PLANE, 1950

Date	Place where released	Time	Distance (miles) and direction from loft	Wind velocity and direction (m.p.h.)	Sky	Visibility	Orientation flight	Time observed	Observation concluded
July 5	Bristol Airport	7:05 P.M.	9 NE	none	complete overcast	good	4 min.	24 min.	at loft
July 6	Shoreham	5:48 P.M.	10 SW	5 W	few cumulus	unlimited	25 min.	43 min.	lost in flight
July 10	Little Notch Road	6:43 P.M.	6.5 NE	slight S	complete overcast	unlimited	—	27 min.	at loft
July 14	Leicester Corner	11:09 A.M.	10 S	slight N	cumulus clouds	unlimited	3 min.	18 min.	at loft
July 14	Leicester Corner	11:35 A.M.	10 S	slight N	cumulus clouds	unlimited	6 min.	19 min.	at loft
July 15	Lincoln	11:20 A.M.	7.5 NE	slight S	cloudless	unlimited	2 min.	18 min.	at loft
July 15	Lincoln	11:45 A.M.	7.5 NE	slight S	cloudless	unlimited	3 min.	28 min.	at loft
July 17	Waltham	11:30 A.M.	10 NW	5-10 S	light overcast	poor; haze	7 min.	21 min.	flock landed; abandoned
July 17	Waltham	10:55 A.M.	10 NW	5-10 S	light overcast	poor; haze	15 min.	33 min.	at loft
July 18	Snow Bowl	10:57 A.M.	10 SE	5 SW (loft) 15-20 (release point)	low overcast	poor; haze	21 min.	33 min.	lost in flight
July 18	Snow Bowl	11:38 A.M.	10 SE	5 SW (loft) 15–20 (release point)	low overcast	poor; haze	5 min.	21 min.	at loft
July 19	Lower Granville	10:57 A.M.	17 E	5 SW	many cumulus	unlimited	5 min.	44 min.	at loft
July 19	Lower Granville	11:54 A.M.	17 E	5 SW	many cumulus	unlimited	2 min.	34 min.	at loft
July 20	Charlotte	11:21 A.M.	20 N	5 N	overcast	good	12 min.	39 min.	at loft
July 20	Charlotte	12:15 P.M.	20 N	5 N	overcast	good	9 min.	33 min.	at loft
July 22	West Bridgewater	10:34 A.M.	35 SE	slight SW	cumulus clouds	unlimited	6 min.	1 hr., 13 min.	at loft
July 22	West Bridgewater	12:09 P.M.	35 SE	slight SW	cumulus clouds	unlimited	11 min.	1 hr., 12 min.	at loft
July 26	Proctorsville	3:21 P.M.	50 SE	15 SW (Rutland)	cumulus clouds	unlimited	14 min.	2 hr., 19 min.	flock landed; abandoned
July 30	Shoreham	10:48 A.M.	10 SW	5 SW	light overcast	poor; haze	12 min.	39 min.	at loft
Aug. 1	Champlain Bridge	6:37 P.M.	13 W	slight N	cloudless	unlimited	3 min.	33 min.	at loft
Aug. 1	Champlain Bridge	7:20 P.M.	13 W	slight N	cloudless	unlimited	0	12 min.	lost in flight
Aug. 4	Proctorsville	3:21 P.M.	50 SE	slight NW	overcast	unlimited	14 min.	1 hr., 14 min.	flock landed; abandoned
Aug. 6	Springfield Airport	11:42 A.M.	56 SE	slight NE	few cumulus	unlimited	20 min.	1 hr., 5 min.	flock landed; abandoned

TABLE 1—*Continued*

Date	Place where released	Time	Distance (miles) and direction from loft	Wind velocity and direction (m.p.h.)	Sky	Visibility	Orientation flight	Time observed	Observation concluded
Aug. 7	Bristol Airport	2:53 P.M.	9 NE	none	cloudless	unlimited	0	15 min.	at loft
Aug. 8	Springfield Airport	12:05 P.M.	56 SE	5 N	few cumulus	unlimited	15 min.	20 min.	lost in flight
Aug. 11	Fair Haven Airport	4:02 P.M.	28 SW	10 N	cumulus clouds	unlimited	18 min.	28 min.	flock landed; abandoned
Aug. 14	Fair Haven Airport	11:45 A.M.	28 SW	5 N	cumulus clouds	unlimited	20 min.	1 hr., 2 min.	at loft
Aug. 16	Westport Airport	11:53 A.M.	16 NW	10 S	complete overcast	poor; haze	1 hr., 7 min.	1 hr., 41 min.	at loft
Aug. 16	Marcy Airport	5:53 P.M.	35 NW	5–10 S	complete overcast	poor; haze	23 min.	2 hr., 5 min.	abandoned because of darkness
Aug. 18	Westport Airport	11:59 A.M.	16 NW	5 N	light overcast	5 miles	23 min.	23 min.	flock landed; abandoned
Aug. 21	Springfield Airport	11:08 P.M.	56 SE	12–15 NW	cloudless	unlimited	8 min.	2 hr., 5 min.,	at loft
Sept. 2	Keene Airport	12:55 P.M.	87 SE	5–10 NW	cloudless	unlimited	11 min.	3 hr., 22 min.	abandoned in flight 3 miles from loft
Sept. 6	Keene Airport	12:29 P.M.	87 SE	5 NW	few cumulus	unlimited	14 min.	49 min.	lost in flight
Sept. 13	Massena Airport	11:37 A.M.	106 NW	20–25 NE	cloudless	haze, then unlimited	8 min.	4 hr., 5 min.	birds landed; abandoned
Sept. 30	Bristol Airport	3:30 P.M.	9 NE	none	clear	5 miles	5 min.	10 min.	lost in flight
Oct. 7	Ballston Spa Airport	12:33 P.M.	75 SW	5 SW	light overcast	unlimited	15 min.	3 hr., 20 min.	lost in flight

The author was observer on all except the last flight, which was observed by Prof. B. F. Wissler.

TABLE 2

FLIGHTS OF ONE- AND TWO-YEAR OLD BIRDS NOT FOLLOWED BY PLANE, 1950

Date	Place or release	Time	Distance and direction (miles)
July 1	New Haven	10:16 and 10:26 A.M.	4 NE
July 2	New Haven	6:25 and 6:43 P.M.	4 NE
July 3	New Haven	10:55 A.M.	4 NE
July 4	New Haven Mills	7:29 and 7:45 P.M.	6 NE
July 5	New Haven Mills	10:25 and 10:29 A.M.	6 NE
July 6	East Middlebury	11:00 A.M.	3.5 S
July 7	Addison	11:15 A.M.	8 NW
July 8	Farmingdale	11:35 A.M.	5 S
July 10	Weybridge	10:46 A.M.	6 NW
July 12	East Middlebury	11:15 and 11:22 A.M.	5 SE
July 12	West Cornwall	7:41 and 7:49 P.M.	5 W
July 21	Lake Dunmore	—	9 S
Aug. 12	New Haven	6:15 P.M.	7 NW
Aug. 13	Middlebury	—	2 S
Aug. 15	Bridport	7:30 P.M.	8 W
Aug. 18	Ripton	7:20 P.M.	6 SE
Aug. 26	Middlebury	7:12 P.M.	2.75 E
Aug. 27	Middlebury	5:38 and 5:48 P.M.	2.75 E
Aug. 31	Middlebury	10:25 A.M.	1 W
Sept. 7	Lake Dunmore	11:00 A.M.	10 SE
Sept. 8	Warren	11:15 A.M.	15 E
Sept. 10	Rochester Gap	4:45 P.M.	23 SE
Sept. 16	Bristol	3:00 P.M.	7 NE
Sept. 17	Rutland Airport	11:20 A.M.	35 SE
Sept. 20	Burlington	—	34 N
Sept. 24	Keene, N. H.	8:50 and 9:20 A.M. (P.M.)	87 SE
Oct. 24	Felchville		35 SE

tion flights appeared indifferent to other pigeons, but on one occasion part of a flock that had just become disorganized joined other pigeons, following them to their loft, 15 miles in a direction almost opposite to that in which they had been flying as well as opposite to the direction of their own loft.

Many flights passed over or close to cities and towns without much indication that the birds were attracted to them. On one orientation flight a flock seemed attracted to a small city nearby, and one flock on a bad course briefly circled a city. Highways and railways exerted no detectable influence.

TRAINING AND EXPERIMENTAL FLIGHTS

Flights can be considered under two headings: training flights, in which the birds were usually released at increasingly greater distances from the loft, and experimental flights, which were designed to show homing ability when the birds were re-

FIG. 4. Training flights nearest Middlebury. Numbers denote month and day. Isolated circles denote flights not followed.

FIG. 5. Training flights from easterly points near Middlebury, drawn on topographic sheets. These flights are also shown in figure 4.

leased in what was assumed to be totally unfamiliar territory. The experimental flights had a second purpose: to see whether directional training would condition pigeons to fly in a particular compass direction.

Many of the flights were in territory the birds had already flown over. Because of the apparent confusion of the birds on the first releases in 1950, as well as our own need to gain skill in following them by plane, a conservative program with respect to distance was followed. Tables 1 and 2 give data on all flights. Figures 4–7 show training flights observed by plane; figures 8–10 show experimental flights.

CHRONOLOGICAL ACCOUNTS OF TRAINING FLIGHTS

The first five flights were not followed by plane but are described because of the birds' apparent confusion in homing from points close to the loft.

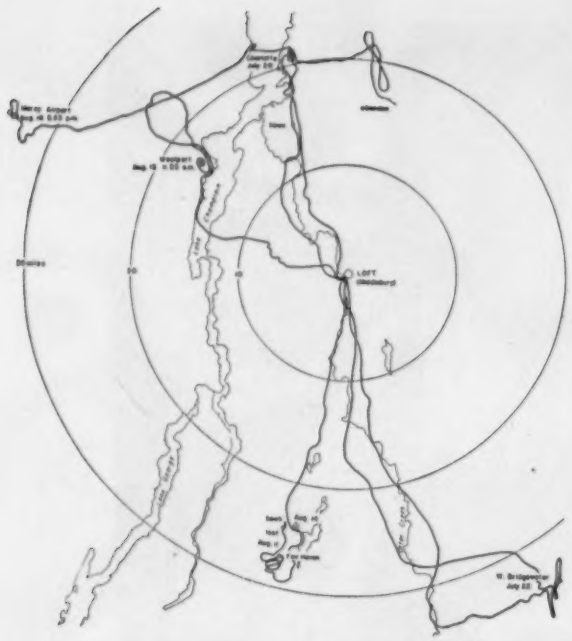

FIG. 6. Training flights, 16–35 miles from Middlebury.

On July 1 the first release was made 4 miles northeast of the loft. The first group spent 10 minutes in the vicinity before disappearing, but joined the second a few minutes later, when it was released. The combined flock remained in sight 10 minutes, then took 10 minutes to reach the loft. The next day they were again released at the same place, and took even greater time before leaving the area. The following day they were released for a third time at the same place, but with no improvement. On July 4 and 5 they were released 6 miles northeast of the loft. On July 5 the orientation flight lasted but 6 minutes.

July 5. 9 miles northeast, at Bristol Airport. All birds were released together and followed by plane. After 4 minutes in orientation flight the flock headed west 5 miles, then south to the loft. Although the first five releases had purposely been made on the direct route to the airport, this roundabout course indicated no benefit from the experience.

July 6. 10 miles southwest. All birds were released together, remaining in the vicinity for 25 minutes. After leaving the area their erratic course indicated confusion, culminating in an attack by a hawk. All returned to the loft within 3 hours.

July 10. 6.5 miles northeast. After a good start the birds headed away from the loft, passing over the place where they had been released July 4 and 5. Shortly afterward a hawk dispersed the flock. The birds followed flew a few miles and then perched. After 5 minutes they resumed flight, homing in almost a straight line from the northeast. This was the only instance in the entire summer when the birds were seen to approach the loft from other than a westerly direction.

July 14. 10 miles south. Each of the two groups circled west about a mile to the railroad and river, then turned north. Although the stimulus to turn toward the loft seemed to be either the railroad or river, the birds followed neither closely, except one flock that made a sharp jog corresponding to the course of the river near Middlebury.

July 15. 7.5 miles northeast. The first flock circled at once to the west, crossed a high ridge although within a short distance of a pass, then headed across lower land toward the loft. The second flock circled elevations to the northwest until reaching the steep western edge of the mountain, which they followed north 2.5 miles to its end. Here they reversed their direction, flying south along the edge of the ridge somewhat past the region of the loft before correcting their course.

July 17. 10 miles northwest. The first flock headed south, but following a hawk attack the birds perched and were abandoned. The second flock ranged north 2 miles and circled the city of Vergennes 9 minutes before taking a fairly straight course home.

July 18. 10 miles southeast, at Snow Bowl. For 13 minutes the first flock flew in the immediate neighborhood, then for 8 minutes circled westward. These birds were lost just after they had crossed Lake Dunmore and were descending into the Champlain valley. The second flock spent but 5 minutes in orientation flight, then took a good course home. The poor showing of the first flock may have been due to low clouds covering nearby peaks, though the same condition prevailed when the second group was released. The birds had been released at the Snow Bowl in 1949.

July 19. 17 miles east, at Lower Granville. The release point was in a narrow north-south valley across some of the Green Mountains from Middlebury. The first flock passed close by the Snow Bowl, flying southwest, without altering their course. They headed toward the loft upon reaching Otter Creek. The second flock flew north in the valley several miles, then crossed westward, somewhat south of the loft. A correction in course was made before reaching Otter Creek.

July 20. 20 miles north, at Charlotte. Both flocks took good courses toward the loft after orientation flights of 11 and 9 minutes, respectively.

July 22. 35 miles southeast, at West Bridgewater. The birds were released near the intersection of two deep, narrow valleys. The first flock spent 6 minutes flying up and down a north-south valley, then climbed in wide circles the mountains to the west. They entered the valley of Otter Creek 17 miles south of the loft, and as soon as they had crossed the river abruptly shifted their course from west to north. The second flock, after 11 minutes of orientation flight, climbed the mountains to the west, passing within a few hundred feet of the top of Pico Peak, elevation 3,967 feet. They crossed Otter Creek 14 miles south of the loft. The turn toward the loft was gradual, beginning while in the Otter Creek valley.

July 26. 50 miles southeast, at Proctorsville. The birds were released a few minutes after their arrival by railway express. They headed west, crossing the valley of Otter Creek without change of direction. The flock was rather loose, though intact until broken up by a hawk. Fourteen minutes after the first attack a second was made. Two birds rose to the level of the plane, then in a few minutes the flock, somewhat reduced in size, settled in a plowed field, where they remained 42 minutes until abandoned. This spot was about 50 miles south of the loft. By July 30 all but 5 of this flock were back at the loft.

July 30. 10 miles southwest. The birds had homed poorly from this place July 6. The flock spent but 12 minutes before starting the homing flight. The course was too far to the south, but a correction was made before reaching Otter Creek. The homing performance was distinctly better than the first.

August 1. 13 miles west, at Lake Champlain Bridge. The first flock left the cage reluctantly, probably because they had not become light adapted following their ride. They flew in the area about 3 minutes, then headed southeast, correcting their direction upon approaching Otter Creek. The second flock made no orientation flight, but started off at once, taking the same route as the first flock for several miles, like dogs following a scent. Unfortunately this flock was lost.

August 4. 50 miles southeast, at Proctorsville. This flight was made by the first 9 birds to return from Proctorsville on the July 26 flight. Their course was similar to the first, but somewhat to the north of it. A hawk attack just as the birds entered the Otter Creek valley caused them to retrace their course a mile, at which point they perched and were abandoned. The one bird that failed to home was later reported at Granville, N. Y., some 16 miles southwest of the place where the hawk attack occurred, and within a few miles of the place where the birds had stopped flying on July 26.

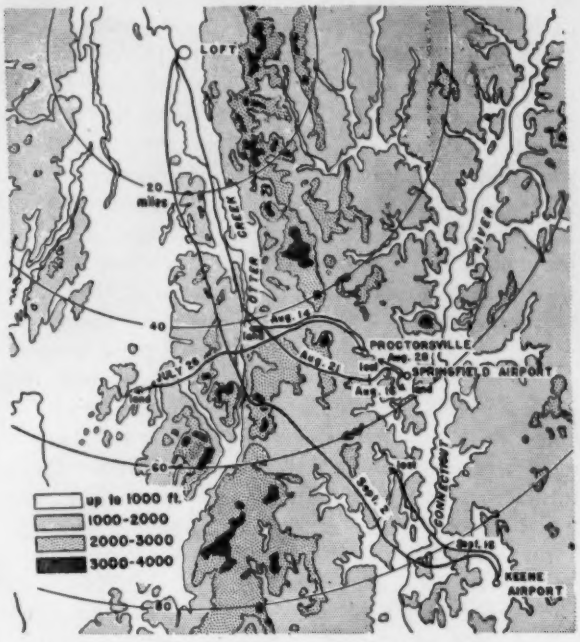

FIG. 7. Training flights, 50–87 miles from Middlebury. Elevations from aeronautical chart.

August 6. 56 miles southeast, at Springfield (Vermont) Airport. Thirteen birds, all of which had made the July 26 flight from Proctorsville, composed the flock. They were transported by plane to the release point—their first plane trip. Within a few minutes after their arrival they were released. An orientation flight of 20 minutes ended when they perched. After 45 minutes they were abandoned; 6 returned within about 4 hours; 5 others returned by August 8; 2 were lost.

August 7. 9 miles northeast, at Bristol Airport. The birds started off almost immediately, taking a shorter route and reaching the loft in better time (15 minutes) than they had on the July 5 flight (24 minutes).

August 8. 56 miles southeast, at Springfield Airport. The birds were rested 30 minutes after

arriving at the airport. Their orientation flight lasted 15 minutes, after which they climbed a steep ridge to the west, and eluded pursuit.

August 11. 28 miles southeast, at Fair Haven Airport. The birds remained near the airport about 20 minutes, then headed north, but were soon lost, apparently dispersed by a hawk.

August 14. 28 miles southwest, at Fair Haven. None of the birds in this flock had been released previously at Fair Haven. They flew rather widely near the airport for 20 minutes, but then took an excellent course home, with no losses.

August 16. 16 miles northwest, at Westport (New York) Airport. The airport is near the west shore of Lake Champlain, at this point 1.7 miles wide. The birds seemed to sense the direction of the loft within a few minutes after their release, often approaching the water but not venturing over it. After 37 minutes the flock headed westward, away from the lake, making a circuit that brought them back to the Westport area again in 20 minutes. This time they were bolder, and in a few minutes all but 3, which turned back, crossed the lake on an excellent course; 2 birds failed to home.

August 16. 35 miles northwest, at Marcy (New York) Airport. This flight was made at the suggestion of pilot Hunt to see whether the birds would cross Lake Champlain without difficulty if they had already set a course before reaching it. The Marcy Airport is some 18 miles west of the lake, in a valley surrounded by peaks 3,600–5,300 feet in elevation, country more mountainous than the birds had previously experienced. The 10 birds in the flock included none of those in the morning flight from Westport. After about 23 minutes of orientation flight, some of it 600 feet above the valley floor, the flock split, 5 birds apparently heading south along a valley, and 5, which we followed, climbing a mountain to the east. One bird dropped out soon; the others flew high up along the mountainside, about a mile from a conspicuous pass at a much lower elevation. At Lake Champlain they hesitated slightly, then crossed. Having set a course apparently enabled them to overcome their reluctance to venture across the lake, here 2.5 miles wide. Their path now led them over Charlotte, where they had been released July 20. At this point they turned abruptly toward the loft, but after a short distance resumed their eastward course. A few miles beyond Charlotte they again headed toward the loft, but only for a few miles. Then they reversed their direction, flew north somewhat beyond their original turning point, finally heading again toward the loft. They were abandoned 16 miles north of the loft because of darkness. No birds homed that day; 6 returned the next day; 4 were lost.

August 18. 16 miles northwest, at Westport Airport. Only birds that had not been released west of Lake Champlain were flown. Like the birds flown on August 16 these exhibited an awareness of the direction of the loft, but a reluctance to venture over the lake. After 23 minutes of flight near the shore and 2 short sorties over the water they perched. All returned later in the day.

August 21. 56 miles southeast, at Springfield Airport. Of the 16 birds in this flock 9 had homed previously from Springfield. They flew near the airport 4 minutes, then headed west. The flock gradually corrected its course toward the loft, following high ground most of the way. Ten homed together; 5 came later in the day; 1 was lost.

September 2. 87 miles southeast, at Keene (New Hampshire) Airport. The 13 birds in this flock remained near the release point 12 minutes, then headed west. As in previous flights from the southeast, the route was at first too far to the south, but it was gradually corrected. On this flight the flock cleared some major elevations by spiral flight before reaching them, almost outclimbing the plane. At another time, while climbing a steep mountain, they zig-zagged just above the trees. Several of the birds, apparently exhausted, perched in a tree near the top. Only 5 birds homed the same day; 2 failed to return.

September 6. 87 miles southeast, at Keene Airport. Fourteen birds were released, none with previous experience at Keene. Unlike the flock on September 2, which climbed from the valley at Keene by spiraling, these birds climbed by straight flight. After reaching the top of the elevation west of the city the flock circled once and split, one loose group heading west, the other, more compact group heading northwest. The latter group was followed half an hour, then was lost. Five birds returned together, with 3 more later in the day; 3 were lost.

DISCUSSION OF TRAINING FLIGHTS

The flights described above were made either in territory that the birds had already flown in or, as the season progressed, not very far from previous release points. The biggest jump between

points was 31 miles, from Springfield Airport to Keene Airport.

The behavior of the birds upon release, except in rare instances, like the second at the Champlain Bridge, August 31, suggests that each release presents a problem in orientation to be solved while flying nearby. The analyses were well made as to the general direction of the loft. In flights where the original analysis was not accurate, corrections were usually made after a period of flight away from the release point.

For flights at the start of the season in territory where the birds had had one or two years of experience one might assume that the orientation flight was to enable the birds to spot landmarks. Observations make this assumption unattractive. In spite of the fact that on several releases like those at the Bristol Airport and the Champlain Bridge, landmarks in Middlebury—Chipman Hill or Middlebury College buildings—could be seen by the observer in the same position as the birds, the direction flown was at an angle to them. No environmental peculiarities were noted that would seem to have influenced them from taking more direct routes. On the flights of July 1–5 within 6 miles of the loft the orientation flights seemed longer than would have been necessary if merely for the purpose of locating known landmarks.

On several releases such as those at West Bridgewater, Proctorsville, and Marcy Airport the birds set their course while still within a valley, before rising to an altitude where distant landmarks might have been seen. The fact that the birds do not customarily rise very high during orientation flights, and home at about the height of treetops, gives little support to the theory that the homing course is determined primarily by recognition of landmarks. The obvious utilization of vision is in enabling the birds to modify their course so as to keep them above preferred terrain and to enable them to anticipate elevations in their path. Geographical features exerted a greater influence on the homing route in the flights at the beginning of the observations than later in the season. Sometimes, as on the flights of July 14 and the first flight from West Bridgewater, July 22, the effect was to orient flocks on a bad course. At other times, like the second flight on July 15, the effect was to direct a flock away from the loft. On the longer flights corrections in course were made gradually, without any obvious environmental cues.

The Marcy Airport flight, August 16, seemed

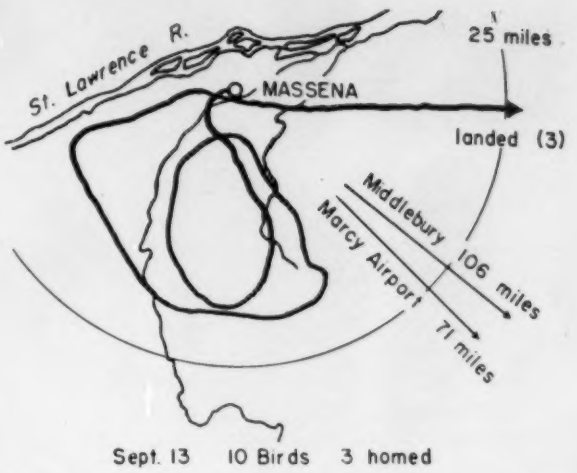

FIG. 8. Experimental flight of Middlebury birds from Massena Airport.

to demonstrate the operation of a strong impulse to fly in a particular direction. Unlike the birds in the morning flight from the Westport Airport which spent almost an hour in the air before venturing across Lake Champlain, the birds on the Marcy Airport flight scarcely paused when they reached the lake. Even the recognition (apparently) of a place from which they had homed less than a month previously failed to change their poor course for more than a short distance. What finally caused these birds to abandon their poor course and fly first toward the loft, then away from it, and finally toward it again? Was it memory of territory seen the previous year, or perhaps the cumulative effect of two areas they had flown over previously? Or was there some other cause? Nothing in the environment suggested the reasons for the changes in course.

A peculiarity, probably of little significance, was that all but one flight of both young birds and old approached the loft from a westerly direction. On two flights of Fort Monmouth birds followed to the loft birds released to the west approached from the north. Mr. Meyer said he thought this might be habitual with the Fort Monmouth birds.

EXPERIMENTAL FLIGHTS BY MIDDLEBURY BIRDS

The longest and most frequent flights were from the southeast. Two flights were then made from other directions, in territory presumed to be totally unfamiliar to the birds. The first of these, on September 13, was from the Massena (New York) Airport, 106 miles northwest of the loft,

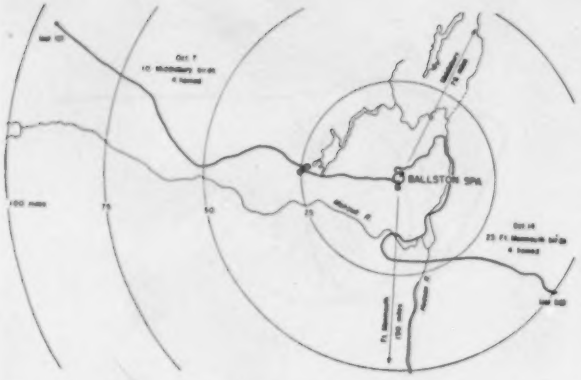

FIG. 9. Experimental flights of Middlebury and Fort Monmouth birds from Ballston Spa Airport.

and 71 miles from the Marcy Airport, the nearest point from which the birds had been flown. Massena lies in the broad valley of the St. Lawrence River. To the southeast, in the direction of the loft, the Adirondack Mountains were dimly visible at first, but more distinct as haze disappeared during the flight. At 11:37 10 birds were released. After 8 minutes in the vicinity of the airport they struck out to the south, then veered to the west (fig. 8). After 45 minutes the flock rose high, circled a few times, and broke up. Some of the birds appeared to continue in a westerly direction. The 4 followed flew north, then east, making a circle about 9 miles in diameter, and crossing their initial route at 12:53 a short distance south of Massena. Then another great circle was flown, about 17 miles in diameter, terminating in a fairly straight easterly flight of 30 miles. After 4 hours and 5 minutes of flight the 3 birds remaining under observation perched. While on an easterly or southerly course the birds flew at a low altitude, but when headed west or north they rose to about 1,000 feet. One conspicuously marked bird had on the original flight south from the airport kept consistently out on the left flank, apparently wanting to fly in a more easterly (and truer) direction. When the second circle was being flown, the bird repeated this performance. On the final eastward flight three birds were left, one of which kept consistently on the left wing, toward the north, one of which consistently kept on the right wing, toward the south, and the conspicuous bird, which now maintained a neutral position, in the center. No birds returned the day of release; 1 returned the next day, and 2 on the second day. The conspicuous bird and 6 others failed to return.

On October 7, 10 birds were released at the Ballston Spa (New York) Airport, 75 miles southwest of the loft, and 48 miles from Fair Haven Airport, the nearest point from which they had been flown. After 11 minutes the flock disintegrated while still near the airport; 22 minutes after their release only 4 birds were under observation, flying westerly. After 25 miles, at Gloversville, 2 of these birds dropped out, and the other 2 headed toward Middlebury—but not for long. Following several minutes of circling a few miles from Gloversville they resumed a westerly course, and were lost about 120 miles west of the loft (fig. 9). Two birds returned the same day, and 2 the next day; 6 were lost, one of which was later reported from Lowville, New York, 15 miles north of the place where the 2 birds were last seen.

DISCUSSION OF EXPERIMENTAL FLIGHTS BY MIDDLEBURY BIRDS

The Massena flight was unique in that the birds seemed to employ a spiral type of exploration. It should be noted, however, that this was abandoned while they were still on the far side of the release point in favor of a long, straight flight about 45° from a true course. Although both the initial flight from the release point and the final straight flight were fairly well directed, the behavior on the whole suggested confusion.

The Ballston Spa flight suggested that the flock had two types of homers in it. Several of the first birds to disappear headed more or less toward the loft. These birds, it may be thought, had analyzed the position of the loft and left the rest of the flock to fly toward it. The greater number of birds, however, flew in the direction in which they had done most of their previous long distance flying. Even the 2 birds followed farthest headed toward the loft for a few miles after being deserted by the others, though finally resuming the direction in which they had been trained. In the case of these 2 birds there seemed to be a conflict between the desire to fly in the direction of previous training and the desire to fly toward the loft, just as there was apparently a division among the members of the flock as to the direction to take.

YOUNG BIRD FLIGHTS, MIDDLEBURY FLOCK

Only 5 flights of young birds were followed by plane. Releases at 2 points from which the older birds had been followed are of particular interest,

Bristol Airport and the Snow Bowl. There was a rather marked similarity in the routes taken by both groups from Bristol. On the Snow Bowl release 2 flocks of youngsters flew more direct routes than the old birds.

FLIGHTS BY FORT MONMOUTH YOUNGSTERS

Five releases of young birds trained to home to the Signal Corps lofts at Fort Monmouth, New Jersey, were followed. Two releases were made close to the route over which they had been trained; the others were made at an angle of some 90° from the direction of training. It was hoped that such observation might indicate whether birds trained exclusively in one direction home by memory of the direction flown or by an analysis of their position each time they are released.

The Fort Monmouth birds, being but a few months old, had had less experience with respect to time than the one- and two-year old Middlebury birds. They had had less training in homing from different directions than the Middlebury birds. Because these birds had been trained by professional handlers it can be assumed that they had been well trained for racing. Mr. Otto Meyer, Chief of the Pigeon Breeding and Training Center at Fort Monmouth, supervised their selection and reported that some of the best birds of the season, as demonstrated by homing performance, were in the flocks followed.

WURTSBORO FLIGHTS

The first flight was made with 20 birds on August 22 from the Wurtsboro-Mamakating (New York) Airport, 90 miles north of Fort Monmouth. The airport is in a narrow north-south valley. The birds at this time had been trained in a westerly direction from their loft as far as Quakertown, Pennsylvania, a distance of 75 miles. They spent 4 minutes in orientation flight, then climbed the eastern wall of the valley and took a southeasterly course that brought them to the Hudson River, which they crossed without hesitation a few miles below West Point (fig. 10). Then they headed in a southerly direction, re-crossing the river at Dobbs Ferry on a good course toward the loft. At Perth Amboy, 20 miles from the loft, the flock was scattered, presumably scared by flashes from arc welding at a tank farm beneath them. After brief circling, 9 birds crossed to Staten Island, where they flew rather erratically

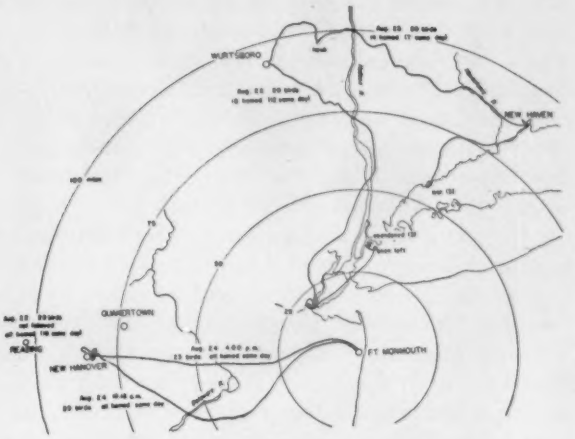

FIG. 10. Experimental flights of Fort Monmouth birds from Wurtsboro-Mamakating Airport, and training flights from New Hanover Airport.

several minutes until joined by 9 other birds. Their course now took them northward along the shore, across the Narrows to Fort Hamilton, and then across Brooklyn to Flushing. where some of the birds landed at a loft. The rest, undoubtedly the Fort Monmouth birds, flew about outside the loft; 3 of them flew west as far as the East River, but turned back and were abandoned while flying aimlessly over Brooklyn.

The rest of the Fort Monmouth flock had apparently continued on a good course after the confusion at Perth Amboy, for they arrived at the loft shortly afterward. It had been planned to have another flock of Fort Monmouth birds, similarly trained, followed the same day from the Reading (Pennsylvania) Airport, 100 miles in the direction of training. Weather at Reading was bad, and the birds were not followed. They arrived at the loft several hours after the first birds from Wurtsboro. Of the Wurtsboro flock all but 2 returned within a few days.

On August 23, 20 more young birds trained like those on the preceding flight were released at the Wurtsboro-Mamakating Airport. Mr. Otto Meyer acted as co-observer. The birds spent 20 minutes in orientation flight, then headed north in the valley (fig. 10). After 13 miles they crossed the east ridge and seemed to be starting on a southerly course down the Hudson valley when attacked by a hawk. After the attack 17 birds pursued an easterly course, crossing the Hudson at Poughkeepsie. The first signs of indecision, circling and looser formation, were noticed 140 minutes after the start of the flight. An hour

later 3 birds left the flock, flying south. Ten minutes later, at Derby, Connecticut, the flock circled. Three birds flew down the Housatonic River in a southerly direction, 4 were left circling, and 3 continued their easterly direction to New Haven. After some circling over the waterfront these birds flew westward along the shore to Bridgeport, where they detoured inland several miles, reaching the shore again at Stamford. Here they were lost, after being observed 4 hours and 45 minutes. For about 15 miles the flock had appeared to follow the Housatonic River, though the 3 birds followed after the disintegration at Derby continued flying in the direction they had been maintaining rather than in the changed direction of the river. From this flight no birds returned the same day; 6 failed to return.

NEW HANOVER FLIGHTS

Two flights were made from the airport at New Hanover, Pennsylvania, 85 miles west of the loft, to observe the homing behavior of Fort Monmouth birds from the direction in which they had been trained. These birds, like those on the Wurtsboro flights, had all homed from Quakertown, about 15 miles northeast of New Hanover (fig. 10).

The first flock, of 20 birds, was followed in the morning. It spent 20 minutes in orientation flight, then took a course somewhat south of the loft direction, homing in 3 hours and 15 minutes. The second flock, of 23 birds, was followed in the afternoon. It spent less time in orientation flight, took an almost straight course to the loft, reaching it in 2 hours and 38 minutes. It is interesting to note that the weather was so threatening that Mr. Meyer was reluctant to let this flock be released. Although there was much haze, there was no rain.

BALLSTON SPA FLIGHT

Twenty youngsters, trained to the west of Fort Monmouth as much as 250 miles, were released at the Ballston Spa Airport, 190 miles north of their loft on October 14. The flock took an erratic course, flying some 15 miles to the north, then south about 35 miles (fig. 9). Finally it headed in an easterly direction, flying at about 1,000 feet elevation. At the Hoosac Mountain range in western Massachusetts the birds were lost from sight when they outclimbed the plane, at that time 800 feet above the mountain. From this flight only 4 birds returned to the loft, none of them on the first day. The flight had been scheduled for October 7, when the Middlebury birds had been released at Ballston Spa, to see whether the two flocks would take similar courses when released within a few minutes of each other. Unfortunately bad flying conditions near Fort Monmouth made it impossible for the Signal Corps birds to be flown to Ballston Spa that day. Figure 9 shows that each flock flew in the direction it had been trained to fly, rather than toward the loft.

DISCUSSION OF FLIGHTS BY FORT MONMOUTH BIRDS

The second flight from Wurtsboro and the flights from Ballston Spa indicate that directional training may upset the homing performance when birds are taken in a strange direction. The fact that the Signal Corps birds flown from Ballston Spa had survived a season of rigorous training, and had (some of them) flown in 250-mile races, marked them as good racers. It is apparent that a distinction should be made between good racers and good homers. The results suggest that, at least while youngsters, pigeons trained directionally are not good homers, able to orient themselves toward the loft no matter in what direction they have been taken.

GENERAL DISCUSSION

RECOGNITION OF LANDMARKS

When birds are released in country they have flown over previously, one might reasonably expect that memory of landmarks would guide their homing flight. In country never previously encountered, where recognition of landmarks is impossible, the problem of orientation becomes more complex. Though some observations on the behavior of homing (and migrating) birds seem inexplicable on the basis of known sense organs, few scientists are willing to admit the inevitability of any other mechanism than visual orientation. Nicol (1945) probably states the view of most pigeon racers when he says that "the homing pigeon normally utilizes landmarks which it has seen before to guide its course." He adds, however, that pigeons can home successfully for long distances from strange territory.

Visual orientation may be of various sorts. First of all is the recognition of familiar landmarks, objects whose position is known by the bird with reference to the loft. Yeagley (1947)

credits trained homing pigeons with such familiarity within a radius of 25 miles of the loft. Visual cues that might be utilized by a bird beyond range of familiar territory might be of a meteorological, geographical, or ecological nature. Some of these are discussed by Griffin and Hock (1949).

When pigeons are released away from the loft, they typically fly in the vicinity several minutes in what seems to be an orientation flight. One might assume that during this period of orientation a search is being made for familiar landmarks. The fact that the orientation flight is made 200–300 feet above the ground does not suggest that the birds are looking for very distant objects. Neither does the altitude of the homing flight itself, usually just above the treetops. When released in deep valleys the birds determine their course before rising to a height sufficient to enable them to see beyond the confines of the valley. Pigeons are capable of flight at higher altitudes; several flocks on bad course flew at an altitude of 1,000 feet.

Many of the releases of the Middlebury birds were made in what may be presumed to have been familiar territory, for the birds were one and two years old and had been flown in several directions from the loft from points up to 32 miles distant. In spite of this experience their behavior, even on the shortest flights, raises doubt that recognition of landmarks plays a major role in homing. On several occasions conspicuous landmarks near the loft were visible to the observer on the ground or at the altitude of the birds. In no case did the pigeons head directly toward these landmarks upon leaving the release point.

The Green Mountains and Adirondack Mountains, both visible from Middlebury, have peculiarities that make them easy to identify. They might be useful aids in orientation, especially if both were visible from the release point. Yet birds released in the midst of each mountain group set their course toward the loft without seeing the other.

On one occasion the birds were flying at right angles to the loft when they entered an area where they had been released 27 days previously. There an abrupt change in direction, toward the loft, but within a few minutes the original, bad course was resumed. Here recognition of familiar territory seems to have occurred, but without benefit in guiding the homing flight.

The apparent confusion of the old Middlebury birds on their first releases and the wide deviations in course on some of the shorter flights are hard to explain on the basis of recognition of landmarks, for the birds were within a few miles of the loft and where at least some of them had flown previously. One might explain such behavior on the basis of their being out of practice following a winter of inactivity. However, poor homing from short distances has been noted in wild birds—gulls (Griffin, 1943), petrels (Griffin 1940*a*), gannets (Griffin and Hock, 1949), and swallows (Wojtusiak, 1949), to mention the most recent reports. Griffin (1940*b*) and Hitchcock and Reynolds (1942) have reported similar experience with bats. Kramer and von St. Paul (1950*b*: 630) note that the releasing of long distance racing pigeons near the loft is considered risky. Although landmarks near the loft were visible to the observer, they were apparently not utilized by the pigeons. On the other hand, on some of the shorter flights topographical features such as river and steep mountainside apparently caused major changes in the direction of flight, not only toward the loft, but also away from it.

If one makes the assumption that a homing bird wants to return at once, poor homing performance at short distances is difficult to explain on the basis of visual orientation involving recognition of landmarks. Such performance is not incompatible with theories of homing based on terrestrial magnetism or Coriolis force. According to these theories the stimuli for orientation at nearby points would be too similar to those at the loft to be differentiated, but those at a distance could be differentiated clearly.

Because the birds had had experience in the region where many of the flights were followed, and because most of the jumps from previous release points to new ones during training flights were less than 20 miles, and the greatest 35 miles, one can suspect that in spite of flight at too low an altitude to permit sight of much more than nearby territory most of the time, and an apparent disregard for obvious landmarks, the birds were nevertheless directing their course by recognition of landmarks. Experimental flights, in which the distance from territory known to have been seen previously by the birds was increased enough to make recognition of landmarks unlikely, were followed. Homing on these flights was affected, probably because of the conditioning to which the birds had been subjected. Although losses were highest on the longest flights, both training and experimental, the initial direction taken on the

longest training flights was good. This suggests that losses on training flights were not the result of poor orientation, but due to lack of endurance and longer exposure to the hazards of flight. Throughout most of the summer flight efficiency of the old birds was reduced somewhat because of molting.

DIRECTIONAL TRAINING

If the homing flight is determined by recognition of landmarks, could a bird be conditioned to fly in a particular direction when released where no familiar landmarks can be seen? It would seem impossible, and Heinroth and Heinroth (1941) and Yeagley (1947: 1062) have reported no conditioning effect on birds directionally trained. Riviere (1929) and Kramer and von St. Paul (1950*b*) have reported a conditioning effect. The experiments of Kramer and von St. Paul were similar to those in this work, and their observations, made without benefit of plane, indicate that pigeons can be trained to fly in a particular direction. It seems possible that the behavior of directionally trained pigeons reported by Gundlach (1932) reflects conditioning, though not so interpreted by him.

In our observations of birds that had been trained in one direction principally and then released in another the homing performance was affected. Five flights of directionally trained birds were followed from strange directions. In three of these flights Middlebury and Fort Monmouth birds showed a distinct tendency to fly in the direction that had previously taken them home. In one of the others an initial course in the direction of conditioned flight was soon corrected. In the other the birds made two great circles and then straightened out on a course some 45° from the direction of the loft. Because the experimental flights of the Middlebury birds were made farther from previous release points than any of the training flights, absence of recognized landmarks may have affected the performance. On one of the flights in which the Fort Monmouth birds that had been trained to the west were released 90 miles north of the loft, several broke away from the flock, in the right direction, after flying 70 miles or more in a southeasterly direction. It seems unlikely that recognition of landmarks could have taken place here. The birds followed farthest on this flight turned toward the loft when they reached the ocean. Was it chance that these birds turned toward the loft instead of away from it? Or was it the same mechanism that had caused the other birds to head homeward before coming in sight of the ocean?

Some light on the mechanism of orientation has recently been reported by Kramer and von St. Paul (1950*a*). Working with starlings they discovered that directional training is possible only when the sky, even when clouded, can be seen. If the same mechanism is utilized by pigeons, vision would seem to be indispensable in homing, the essential stimuli coming from the heavens rather than from terrestrial landmarks.

EXPLORATION

Theories of homing from distances at which recognition of familiar landmarks can be eliminated from consideration are of three types: those that postulate exploration until familiar territory is reached; those that postulate an ability to navigate through the perception of electrical and magnetic forces; and those that postulate a memory of kinaesthetic stimuli received during transportation to the release point. In a different category are the theories that attribute homing to a "homing instinct" or "sense of direction."

Exploration may be of two types, called by Griffin (1944) radial scattering and spiral exploration. In the former it is assumed that the direction of flight from the release point is a matter of chance. Birds that happen to fly in the right direction home; the others will be lost. In spiral exploration the bird is assumed to make widening circles about the release point until it reaches familiar territory. Because the spiral route may be very long exhaustion and lack of persistence may account for failures to home.

The recent aerial observations of homing gannets by Griffin and Hock suggest that an initial radial scattering is followed by a tendency to spiral. This sort of pattern has been suggested for homing pigeons by Gundlach. Heinroth and Heinroth attribute spiral exploration to homing pigeons, but Riviere, whose experiments were similar to those of the Heinroths, denies it. Our observations showed no indication of radial scattering. Following an initial orientation flight the homing course was distinctly in the direction of the loft in training flights. Similar behavior in swallows has been reported by Wojtusiak. These birds when liberated "soar" into the air, make one or more circles, and then typically take a "distinctly directional" course toward the nest. In three out of five of our flights of directionally

trained pigeons when released in strange directions the flight was in the direction that they had previously taken to reach the loft.

In one experimental flight the pattern suggested spiral exploration. After the customary orientation flight the flock flew almost 20 miles at an angle of 20° from the loft, then began veering away from it. The diameter of the first circle was about 9 miles; that of the second was about 17 miles. The second circle was not quite completed, as the birds finally flew a fairly straight course for some 30 miles at an angle of 45° from the loft. Had this been spiral exploration one would have expected the straight flight to commence when familiar territory had been seen. In this case, however, it began on the far side of the release point, which was crossed near the beginning of the straight flight. The spiral pattern seems to have been accidental. The two great circles might be considered as a prolongation of the orientation flight or due to changes in the composition of the flock, which was reduced during the flight from 10 birds to 3.

The homing flights of gannets observed by Griffin and Hock, as well as those of gulls observed by Griffin, had a randomness of direction not seen in the pigeon flights. If one assumes that in at least some of the flights the pigeons were beyond familiar territory, this difference is puzzling. Perhaps the characteristics of flight may be involved. Gannets and gulls are soaring birds. Gannets were observed at 5,000 feet (Griffin and Hock, 1949: 181); gulls at 4,000 feet (Griffin, 1943: 29). Pigeons do not soar; they home with scarcely a break in the rapid beating of the wings. They were not seen at much more than 1,000 feet, and customarily they flew at the level of treetops. Both gannets and gulls appear to seek favorable updrafts. Such search might involve large distances. Reluctance to leave favorable air masses might further complicate their route. The pigeons possibly sought obstructional updrafts along ridges and outcroppings, though their preference for such formations could not be correlated with the direction or velocity of the wind. Nor did this preference alter the over-all direction of flight. The pigeon, unable to take advantage of air currents by soaring flight, might be expected to take a more direct course than gulls and gannets. Unfortunately no gulls or gannets were followed all of the way home, so it is not known under what circumstances their exploratory flight was replaced by flight directed toward the nest.

Another consideration in comparing gannets and gulls with pigeons is that the former are wild birds and the latter domesticated. Pigeons that have never flown outside the loft are said to lack the ability to home. If this is so, and not due merely to weakness resulting from a lack of exercise, the training of the pigeon introduces a factor that may be more variable than in wild birds. As indicated above, directional training dulls the pigeon's ability to orient toward the loft, though enabling it to take the direction for which it has been trained. In fact Kramer and von St. Paul (1950*b*) conclude that the fastest homers in the direction of training are the slowest homers when released in another direction from the loft. Whether daily activities exert a similar effect on the homing of wild birds is not known, although Wojtusiak reports that homing of swallows that follow a north-south migration route is equally good in all directions.

If environmental cues of an ecological or meteorological nature are the effective guides in orienting homing pigeons, they have not become apparent in our studies. Homing flights were made at various times of the day, with winds of different directions and velocities, on days of sunshine, and on days of complete overcast.

MAGNETIC AND ELECTRICAL FORCES

Of the theories postulating a sensitivity to magnetic and electrical forces, that of Yeagley (1947) is perhaps the most attractive. According to this theory the bird responds to electromotive forces induced by the Coriolis effect and the earth's magnetic field, the two forming a grid in which orientation should be a simple matter. Although aerial observation of the homing pigeons failed to yield any evidence that the birds were responding to forces to which the human being is sensitive, no direct evidence was noted that would specifically support Yeagley's theory. Nor was evidence noted that would tend to discredit the theory. Until a sensitivity to the forces postulated by Yeagley has been demonstrated, it is to be expected that the theory will be regarded with skepticism. Some of the criticisms of the theory have been summarized by Odum (1948). A more recent treatment is that by Wilkinson (1949) and other participants at a conference devoted to the problem of orientation in homing and migration, reported verbatim in the *Proceedings of the Linnean Society of London,* **160** (2): 85–116,

1949, and summarized in *Nature*, **161** (4104): 996–998, 1948.

KINAESTHETIC MEMORY

The theory that homing is by memory of kinaesthetic stimuli received during transportation to the release point has not been supported, at least in its simplest form, by these observations. There was no indication that the birds attempted to retrace the route over which they had been carried to the release point.

"SENSE OF DIRECTION"

In the realm of less precise explanation mention should be made of the "sense of direction" attributed to the homing pigeon by Riviere, and to the swallow by Wojtusiak. Riviere, convinced that pigeons home from places they have never been to with greater success than would be possible by exploration, concludes that eyesight can probably be eliminated as a direct factor. Their homing ability he considers to depend on a "sense of direction" that enables them when liberated in a strange locality to maintain the direction in which they have been accustomed to fly. Because pigeons merely exercised around the loft, but never trained by being released at a distance from it were able to home well when released initially at 58 miles, then at 89 miles and 130 miles in different directions, he postulates in them a "sense of geographical location," possibly identical to "sense of direction."

The flights by both Middlebury and Fort Monmouth birds exhibited conformity to Wojtusiak's and Riviere's concept of a "sense of direction." Their behavior suggests that Riviere was right in crediting eyesight with little importance to the pigeon in homing. "Sense of direction" is a good descriptive term, not an explanation. With continued investigation the nature of this "sense" should be reduced to physical dimensions.

SUMMARY

1. Flights of homing pigeons released in flocks at various distances up to 190 miles from their lofts have been observed from an airplane.

2. Following an orientation flight in the vicinity of the release point at an altitude of 200–300 feet flocks home at about the level of treetops. Flocks on a bad course may fly at about 1,000 feet.

3. The homing flight is definitely oriented toward the loft from the beginning, not started in random directions, disregarding the orientation flight, from the release point.

4. Flocks that had been trained in one direction, when released in another direction, gave evidence of having become conditioned. Individual birds in such flocks appeared to sense the direction of the loft, but left the flock with apparent reluctance.

5. The observations are discussed with reference to various theories of homing.

a) Evidence from releases in territory presumed to be unfamilar to the birds, as well as in territory which they had flown over previously, gives no clear support to the theory that orientation is by recognition of familiar landmarks.

b) The flights were not of the sort that gave evidence regarding the influence of magnetic or electrical forces.

c) There was no tendency to retrace the course over which they had been transported to the release point.

d) The pigeons act as though they have a "sense of direction." The stimulus or stimuli and the sense organ or organs involved in this behavior were not apparent.

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AN INVESTIGATION OF CORTICAL CURRENTS

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INTRODUCTION

THE experiments which will be described in this report refer to a problem in brain physiology, and have been done with a physiological technique. The scientists who will have to pass judgment on our findings are therefore the neurophysiologists, and for them our report had obviously to be written. But the question which led to our work in this field has arisen from the comparison of a widely accepted physiological theory with certain facts in the psychology of perception. Thus physiologists will not be able to see why we have thought that this investigation should be undertaken unless they hear what particular psychological evidence is supposed to be relevant to their theory. We will therefore now briefly indicate not only what assumptions about brain function we have found difficult to accept, but also what observations in perception these assumptions do not seem to fit.

The physiological views to which we are referring are those which, fifty years ago, another critic, the physiologist von Kries, called *die Leitungslehre* (17).* By this he meant, of course, the belief that, basically, all functions of the nervous system, including those of cortical tissue, can be understood in terms of two processes: conduction in the fiber of a given neuron and, at its end, events which start conduction in a further neuron. He was aware of secondary concepts which had gradually accumulated around this core; but he found the doctrine just as unable to explain a number of obvious facts when such concepts had been added as it had been before. Characteristically enough, the facts with which it seemed to him to be incompatible were also taken from psychology, namely, from memory or, more precisely, from achievements of recall. His arguments, which later played an important role in Gestalt psychology, appear to us convincing. But, to our knowledge, they had hardly any effect upon further developments in physiology. This may have happened partly because the concept of conduction in fibers which he had found of only limited use in the physiology of the brain was, only some years later, immensely clarified by great discoveries about the nature of the nerve impulse, and thus became the center of general attention. It is perfectly natural that the temporary focus of successful experimentation be also made the focus of theoretical views far beyond empirical knowledge, particularly if such views agree with tendencies already strong before the new encouragement. At any rate, it remained, or now became, the conviction of most that eventually the *Leitungslehre* would give a satisfactory account of *all* achievements of nervous systems.

Most probably, this doctrine appeals to the scientist also for reasons which are not derived from physiological observation as such. If, on the whole, the results of processes which occur in nervous systems, are well-ordered responses to given situations, in what other fashion could this order be mediated? Supposing that the conducting fibers are properly laid out, order as to function would simply follow from a corresponding order in the distribution of the conductors. Processes cannot go astray, and they cannot become mixed, under such conditions. Is not this the only way in which physical events can be forced to take an orderly course? There is no reason to assume that brain processes are exceptions to the general rule.

We doubt whether there is such a general rule. Order of responses to given situations can also be established and maintained in other ways. At any rate, in spite of the fact that so much is now known about nerve impulses, we still believe with von Kries that, when the conduction doctrine is applied to brain processes, results do not agree with psychological evidence. Nor do we see that, in this respect, the situation is essentially changed by the more recent discovery that conduction may occur in circular chains of neurons, or by the fact that more than one impulse must generally arrive at a cell if it is to be activated.

While von Kries' arguments were strong, they did perhaps not touch upon the most fundamental

* Numbers in parentheses indicate references at end of article.

difficulty. This difficulty arises in the psychology of perception rather than among the facts of memory; and, if we are not mistaken, the conduction doctrine cannot adequately deal with these facts mostly because it fails to solve a basic problem in the former field.

The order of processes considered in the conduction doctrine is ultimately of an anatomical or non-functional nature. If the individual conductors were not properly arranged in space, there would be no such order. The doctrine offers no suggestion as to any order in processes which could not be established by keeping local events separate in individual fibers. For instance, in the order of brain events with which the doctrine is concerned, it can hardly matter whether the messages arriving at various points of the visual cortex are all of the same kind. A purely geometrical aggregate of local events would be established under these conditions. But this would also be the case if, within a certain area, impulses were to cause the local processes corresponding to the color green, while all around the messages were those which give rise to a certain achromatic nuance. To be sure, in either situation further impulses might now begin to travel in cortical neurons which extend in various directions, and it would not be surprising if, in the heterogeneous situation which has just been mentioned, some kind of blurring were to result from the fact. But what actually happens in this situation is far from making the impression of a confusion. The visual field neatly splits into two parts: a green form is segregated as a unitary object from an equally continuous achromatic environment. If we may use a terminology which has widely been adopted in psychology, the split establishes a clear *molar* order as distinguished from an orderly arrangement of independent *molecular* events. The fact on which the molar order here depends is, obviously, that color conditions at all points in one region of the visual cortex are the same, while those at all other points are also alike, but different from the first. We cannot discover why in this situation the visual field should split into two molar parts, if the cortical effects of arriving impulses are mutually independent local events, and if, therefore, differences among these events can have no functional consequences. We also fail to see how further impulses, which may now travel in various directions, could cause such a split.

At first, it may not be easy to follow our argument, precisely because likeness of local stimuli within a circumscribed region A always separates this region from its surroundings B, where stimuli are also alike, but differ from those in A. For, as a result, the segregation of specific objects is, as a virtually omnipresent phenomenon, taken for granted not only in visual perception but also in our thinking, even when in the situation about which we are thinking there are no molar entities whatsoever, and when, therefore, thinking in molar terms falsifies objective facts. For instance, one argument of von Kries refers to the observation that the shape of an object is still recognized when the corresponding stimuli are projected upon different parts of the retina, and when the size of the pattern is also considerably changed. It is quite true that such facts cannot readily be reconciled with the *Leitungslehre*. But even von Kries does not seem to have realized that, according to this doctrine, the situation contains no physiological entities to which molar sizes and shapes could be ascribed under any circumstances; and that in this fashion the worst difficulty with which the doctrine is confronted is ignored in his argument. Obviously, we have no right to single out a particular group of local events as a molar unit with a shape and a certain size, if *all* such events are supposed to be indifferent to one another. From the point of view of function, the group would have no reality, however much we may be impressed by a corresponding molar object in the visual field (12, pp. 161 ff.).

Physiologists may not feel inclined to pay much attention to peculiarities of perceptual fields as such. They are likely to be more interested in facts which have biological consequences. We will therefore now turn to effects which segregation of specific objects in visual fields has upon behavior. Actually, some of the most immediate responses of man and the higher animals are concerned with the presence of objects in this sense. For example, movement in a visual field is generally a powerful cause of overt reactions. Quite apart from such psychological consequences of a movement as the involuntary concentration of attention on the moving part, the eyes, but often also the head, and perhaps the whole body, turn toward that part. Particularly the eyes may then be caught by it to such an extent that they are likely to follow its course for some time. But what is the moving "part"? There can be no movement in the visual field unless a particular object is present, the boundaries of which are dis-

placed in relation to other objects or to the observer.

Objects in the present sense do not cause overt reactions only when they move. Upon the sudden presentation of a visual situation in which an object is surrounded by a homogeneous environment, this object will immediately be fixated unless the observer makes a special effort to prevent this response.

Under certain conditions the eyes change the degree of their convergence, or also rotate in relation to each other. The result of such responses is that double images fuse. Obviously, the initial separation of these images is the condition by which the movement is caused, and the separation abolished. But what are the particular facts, the images, which are separated in the beginning? Surely, they are not "stimuli"; for stimuli in the strict sense of the term are present everywhere. Rather, these facts are pairs of similar lines or circumscribed surfaces, that is, of molar objects, and it is against the separation of these that such movements operate.

An important determinant of overt responses is depth perception. But there is hardly any depth perception in the absence of segregated objects, and depth is most accurately perceived as a result of binocular disparities. Now, what facts are disparate under these circumstances? Just as in the case of eye movements by which disparities are abolished, these facts are double images. The spatial relation of these images determines the sign and the amount of perceived depth. But again, such images are not "stimuli." Much too often this term is used not only in its strict sense, but also with reference to segregated objects in the sense of the present discussion, and we have seen that double images are such objects. The problem which arises when the *Leitungslehre* is confronted with the existence of such objects cannot readily be recognized so long as we hide it behind a familiar but ambiguous term.

Actually, the relation between local stimulation and particular objects in the field is by no means simple and direct. For such objects are just as often dark in comparison with their environment as they are bright, and, so far as present knowledge goes, they cause about the same responses or other further events in both cases. The eyes fixate or follow dark objects no less than bright objects. Fusion movements of the eyes are elicited by disparities of dark as well as of bright lines; in fact, most experiments in which such movements have been studied have probably been done with dark lines. The same is true of studies in which the dependence of depth perception on disparities has been investigated. The lines used in experiments of this kind are generally dark lines on a white ground, and, surely, properly chosen disparities give perfectly clear depth under these circumstances. Hence, it cannot be regarded as a plausible assumption that, in this instance and in the others just mentioned, it is nerve impulses as such which are responsible for the existence of particular objects, and thus for their effects. The reversal of brightness relations in a field shifts the locus of intense impulse action from an object to its environment, or *vice versa*. And yet, the object remains the center of further events from the point of view of biology as well as from that of psychology.

Molar objects appear to play the role of areas in which, physiologically speaking, the energy of some function is highly concentrated. This is probably more than a figurative expression if we can trust the evidence found in a recent investigation of so-called figural after-effects (14). In this study it has been shown that visual objects recede from places in which other objects have been seen for some time. It is now a fact rather than an hypothesis that the processes which correspond to molar objects strongly affect their cortical medium. The nature of the change is that of an obstruction. After prolonged inspection, this obstruction can be demonstrated within a certain range around the area of the previously inspected object; but the obstruction is maximal within this area. It seems safe to conclude that here cortical events of some kind have been particularly intense. But what cortical processes are concentrated in this place? And why are they stronger here than in the wide environment? The processes in question cannot be nerve impulses because it has been found that both bright objects in a darker environment and dark objects on a bright ground cause strong effects. Moreover, the obstruction is maximal within the area of the object also when this object is very much darker than the remainder of the field. So long as nerve impulses are taken to be fairly sharply localized events, any obstruction which they might cause ought to be stronger in the brighter parts of a visual situation; but this is certainly not true of the obstruction which is caused by the prolonged inspection of a dark object.

No more evidence will be needed. Apparently, the conduction doctrine cannot explain the splitting of visual fields into molar objects and their

background, and is therefore equally unable to deal with the consequences of this phenomenon in overt action, and in the distortion of subsequent perception. We hope that under the circumstances physiologists will regard our problem as an issue which also concerns their own science.

We have mentioned several reasons which prevented von Kries' contemporaries from paying much attention to his critique. But there was a further reason. The critic did not offer a specific hypothesis of his own by which the conduction theory of brain function might be replaced. We will now formulate such an hypothesis.

A suspicion as to the processes by which specific objects are established might have arisen a long time ago, if the techniques commonly used in neurophysiology had been examined from a theoretical point of view. For the most part, brain events have been investigated with instruments which give a good account of certain phenomena, namely, waves of limited duration, but make it difficult for certain other facts to announce their presence in the records. Apparently, experimenters expected all valuable information to be concerned with such waves, and therefore generally worked with capacity-coupled amplifiers especially constructed for the study of oscillations. As a result, a quiet base line and most satisfactory records of waves were obtained, but evidence of steady or slowly fluctuating states was eliminated. For similar reasons, the stimulation applied in experiments was mostly kept quite short. Thus, even when direct-coupled amplifiers were used, records could not give adequate information about slow processes which might accompany continued stimulation.

If, under the circumstances, a theorist had asked himself what cortical action could be responsible for facts in perception which had so far not been explained, he might profitably have turned toward processes not easily discoverable with customary experimental procedures. For the postulated action would hardly have escaped the experimenters' attention if it could readily be recorded with such techniques. As a next step, our theorist would have realized that the experimental conditions commonly used in neurophysiological work operate against the recording of smooth processes, and that for this reason such processes are plausible candidates for the postulated function. Now, the only smooth process that could be considered in this connection was the electric flow or current. Consequently, the theorist might have asked himself whether such currents could be established in cortical tissue, and also, whether their occurrence would explain the puzzling facts in perception.

Unfortunately, this line of reasoning does not seem to have been followed by anybody. Nevertheless, it was suggested many years ago that the solution of certain problems in psychology might be found if the concepts of what the physicists call potential theory were applied to brain function (9). More specifically, the possible significance of cortical currents was discussed in this connection. But at the time no clear decision seemed possible as to the way in which such currents would be conducted in the tissue. Gerard first answered this question in 1936. He found it necessary to assume that cortical tissue is pervaded by fields which spread in this medium as a continuum (4, 5). He and Libet also reported about experiments which strongly encouraged his assumption (6, 15). It is a remarkable fact that Gerard's views were derived from purely physiological evidence; for we developed the same hypothesis when occupied with the molar characteristics of visual fields, and with figural aftereffects (10, 11, 14). Thus, observations in two sciences now point in the same direction.

In the following, we propose to test the hypothesis that, when nerve impulses arrive in the striate areas, electromotive forces are here established which drive a current through the cortex approximately at right angles to its surface. No attempt will be made to decide whether perhaps these forces merely alter others which are present in the cortex quite apart from impulse action, a possibility that is suggested by Gerard's work. When the whole visual cortex is uniformly affected by nerve impulses, forces of the same intensity will arise in all its parts. Under such circumstances, the only flow which need be considered will everywhere pass through the cortical layers in the same direction, and will then return to its source in loops around the whole visual area. Obviously, the density of a current of this kind must be fairly low, because the cross-section of the circuit is large throughout. But when more impulses arrive in a circumscribed region than reach the environment of this region, and when therefore electromotive forces in the environment are weaker, then the current can take a shorter course. After passing through the area in which the driving forces are stronger, it can now return to this source through the visual tissue around it. The density of such a flow will naturally be greater. It will be greatest within the circum-

scribed region, because this is the narrowest part of the circuit.

There is a second condition under which the flow will have its greatest density in a circumscribed area. If fewer impulses arrive in such a limited area than arrive in all parts of its environment, then the current which now issues from these parts can return through the narrow area with its lower electromotive force, and will here be strongly concentrated, again because the cross-section of the area is so small. We know, however, that regions in which fewer impulses arrive are, psychologically speaking, darker regions than those in which impulse action is more intense. Thus it follows from the present hypothesis that the energy of the cortical flow will be maximal in circumscribed areas whether, in a psychological sense, such areas are brighter or darker than their environment. Obviously, this result agrees with the facts in perception which have been discussed in preceding paragraphs.

There is a further point of agreement. Any smooth current which passes through cortical tissue establishes in this medium the electrotonic condition, and this condition is enhanced as the flow continues. On the whole, electrotonic effects have the character of obstructions. According to our hypothesis, obstructions of this kind must be caused in the visual cortex also under the influence of its own currents. Consequently, when stimulation of the eyes gives rise to such currents, the flow will immediately begin to block its own path, and thus to reduce its intensity. Moreover, currents which afterwards spread in the same region will also be weakened, and at the same time be deflected from the most affected parts. But the most affected parts are those in which the intensity of the first current has been particularly great, in other words, in such circumscribed regions as have been either brighter or darker than their environment. Elsewhere an explanation of figural after-effects has been given in which such effects are derived from the fact that the distribution of visual currents must be distorted if these currents flow in regions in which previous currents have established the electrotonic obstruction (14). The present argument shows that this obstruction must be maximal in circumscribed areas which are either brighter or darker than their environments. Actually, we have seen, the obstructions which cause figural after-effects are maximal in the interior of circumscribed objects, whether these objects are brighter or darker than their environment.

The distribution of visual currents also separates such areas from their environment. For the potential which is established within a circumscribed area differs from that in the surrounding tissue. This follows from the fact that, after passing through the area, the current flows along a path of some length before it returns to the cortical tissue adjacent to the area. This involves a corresponding change of potential. At the common boundary of the area and its environment, different potentials are, therefore, maintained side by side, and thus the spatial continuity of cortical action is suddenly interrupted where the area and its environment are in contact. It is merely another phase of the same separation that, again at the boundary, the direction of the flow is suddenly reversed. The current has one direction inside, and the opposite direction outside. We do not know whether physicists have ever dealt with such an extreme situation, and with the questions of detail which are involved. But the situation is perfectly clear as to the main point. Whether a circumscribed region in the visual field is brighter or darker than its environment, the current within its cortical counterpart will not only be more concentrated than it is in the surrounding tissue; at the level which we are considering, the flow within and the flow without will also be sharply segregated from each other. It is not surprising that under the circumstances the circumscribed part of the visual field appears as a separate object.

When formulating our hypothesis, we have not explained how electromotive forces are supposed to originate when nerve impulses arrive in the visual cortex. There are several ways in which such an explanation can be given. Apparently, however, this issue has no bearing upon the distribution of the currents which such forces would maintain. Moreover, if the existence of the currents as such can be demonstrated, the existence of corresponding electromotive forces cannot, of course, be doubted. We will therefore postpone discussion of this question until it has been shown that currents actually flow in the striate areas when objects appear in the visual field.

One further remark seems indicated at this point. The present hypothesis involves no criticism of generally accepted views concerning the propagation of nerve impulses from the eye to the visual cortex. On the contrary, it is an essential premise of the hypothesis that these views are, on the whole, well established.

Several records which will be reproduced in

this report are further demonstrations of facts already described and shown in other articles (7, 13). Confirmation of our earlier findings has seemed to us to be an essential task. For the thesis that cortical function is largely a matter of field physics will not be accepted unless enough evidence is presented. But this thesis will also be supported by new facts which we have found in the meantime. We cannot yet evaluate the significance of certain other observations which we have to report. This will be possible only after a great deal of further experimental work.

Our investigation has been made possible by grants from the American Philosophical Society, the Viking Fund, and Swarthmore College. We wish to express our gratitude for so much generous help.

I. INSTRUMENTS AND PRELIMINARY TESTS

Since the amplifiers commonly used in electroencephalography did not serve our purpose, it was our first task to find a more adequate amplifying system. Direct-coupled amplifiers seem to require almost constant attention, a great disadvantage in an investigation which is likely to be difficult quite apart from short-comings of the main instrument. Our problem was solved when, several years ago, a breaker-type amplifier became available which gives a perfectly stable account of slowly varying or steady potentials (16).[1] In this machine a first breaker transforms the smooth flow of weak currents into 75 pulses per sec. which, after amplification in the conventional fashion, are given again the form of a smooth flow by a second breaker with the same period. Actually, in the output the frequency of the breakers is superimposed upon the steady flow. These oscillations can, of course, be eliminated; but no particular measure need be taken in this respect if the output is recorded with an instrument which reacts too slowly to give any direct evidence of the presence of the oscillations. We are using a recorder of this type.

The intrinsic frequency of the amplifier is indirectly revealed when, as a consequence of inadequate connections, the 60 cycles of the power supply intrude into the circuit. While our recorder does not register the 75 cycles of the amplifier, it can follow the 15 beats per sec. which originate under these circumstances. Their appearance may be used as a convenient warning signal. For the most part, this signal means that one of the electrodes is not properly attached, or that the input impedance is unduly raised elsewhere.

At very high gain, the amplifier may be used for registering potentials far below the level to which our investigation can refer. With the electrodes placed on the head of a human subject, there are always irregular fluctuations of the base line, the cause of which lies in the organism.[2] Their size varies from one subject to another; but deflections obtained under particular experimental conditions can never safely be related to these conditions unless their size corresponds at least to a few μV. (With a physical source, the amplifier gives an entirely reliable account of much lower potentials.) Drifts which often appear when the head is part of the circuit never originate in the amplifier. They are either of organic origin or caused by defective electrodes.

We register the output of the amplifier with a photoelectric recorder of the General Electric Company. Depending upon the problem under investigation, we use paper speeds of .25, .75, and 6 cm. per set. Different galvanometers may be inserted in the recorder. We are working with two such elements, the response times of which are .04 and slightly more than .5 sec.

Although the slower galvanometer obviously distorts responses whenever the intensity of the current changes with some speed, we often use this galvanometer for the following reason. Records taken with the faster galvanometer are generally covered with waves of the α-rhythm, or with such modified forms of this rhythm as are left under conditions of stimulation. As a consequence, records tend to give a clouded picture of steady or slowly fluctuating currents, particularly when, as in most of our work, the speed of the recording paper must be kept fairly low; for the duration of an individual experiment is often greater than 10 sec., and occasionally as long as 30 sec. With the slower galvanometer, this difficulty does not arise, because it cannot follow the α-rhythm, and thus eliminates it altogether. There are, of course, problems which can be investigated only with the faster galvanometer. For others, even this instrument and the recorder as a whole operate too slowly. We cannot, for instance, adequately register oscillations of more than about 25 cycles per sec., and have therefore made no attempt to deal with questions which refer to such higher frequencies.

For the most part, we have worked with human subjects (*Ss*), and registered such currents as flow into the amplifier through non-polarizable

[1] The instrument can be obtained from the Perkin-Elmer Corporation in Glenbrook, Conn. The impedance of the input transformer in our amplifier amounts to about 200,000 ohm.

[2] We are here assuming that the electrodes are in perfect working order.

electrodes attached to the intact head. Our electrodes are of the silver-silver chloride type.[3] They are placed in glass tubes, and surrounded by a saline solution (10 per cent) which makes contact with the scalp. The diameter of the contact surface has been varied from slightly more than one cm. to about .3 cm. Under the conditions of our present work, there are only few experiments in which differences in this respect seem to influence results. When the electrodes have to be placed quite near each other, it is advisable to give them a smaller diameter. Otherwise, we find it convenient to work with a rather large surface of contact. So far as we can see, no serious objection can be raised to this procedure since, in the investigation of currents which spread through considerable parts of brain, skull, and scalp, questions of localization in detail do not arise in the same sense as they do when, for instance, evoked potentials are registered from points of the exposed brain.[4] One electrode is always connected with the grounded parts of the amplifier, and with the shielding cage in which the *S* is seated.

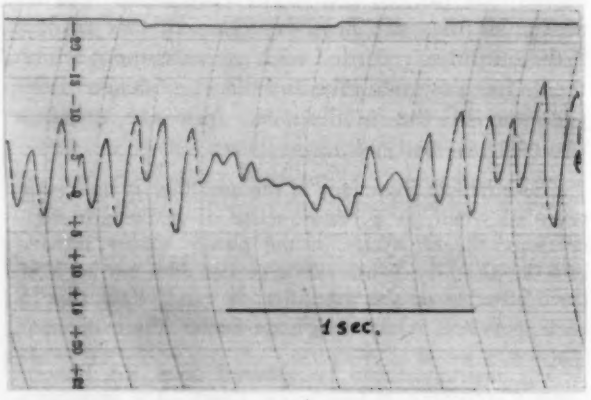
We have never had serious difficulties caused by defective shielding of the *S*. At first, we believed that, in visual experiments, *S*s must see the experimental objects through the network of the cage by which they are surrounded; but records were not appreciably disturbed when a window of considerable size was cut in the cage, through which a wide field could be seen without obstacles. By a comfortable head rest the head was kept quiet in such a position that the face was placed only a few cm. behind the window.

On the other hand, experimenters who wish to work with the same kind of recorder as has been used in our work will have to be cautious when they operate with further electric equipment in the same room. Electric motors, for instance, must be kept at a considerable distance from the recorder. Under all circumstances, it is advisable to make sure that fields of such instruments have no influence upon the recording.

We have often marked the periods of stimulation and the like with automatic devices of one kind or another. But since the nature of our present work does not require that the marking be extremely accurate, we have also frequently marked by hand.

To our knowledge, the combination of instruments with which we are working has never been used in neurophysiological investigations. This holds particularly for the most important part of our equipment, the amplifier. For this reason we will now show records of well-known physiological facts, and thus make it possible to compare these records with those which more familiar instruments give of the same facts. As test cases we choose the α-rhythm, and the blocking of this rhythm by certain factors. When registering the α-waves, we placed the active electrode slightly above the occipital protuberance, i.e., in a place under which foveal parts of the visual cortex are located, and attached the second electrode to the vertex. The paper speed of the recorder was 6 cm. per sec., the response time of the recorder .04 sec.

When Record 1 was taken, the *S* kept his eyes closed in a room which was at first dark. During the period indicated by the deflection of the line on top, a bright circle was projected on a screen before the *S*, so that considerable light fell upon his retinae through the closed lids. The beginning of the record shows the undisturbed α-rhythm (about 10 cycles per sec.). Upon stimulation, the rhythm is blocked within a fraction of a second; but it is soon reestablished, although with a slightly lower frequency, when the light has again disappeared.



RECORD 1. Subject B. The α-rhythm is temporarily blocked by visual stimulation.

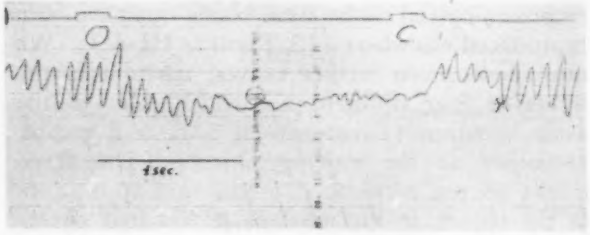
[3] Electrodes of this type are described in Malcolm Dole, The glass electrode, 98–101, Wiley and Sons, 1941. We have also used the technique given in H. S. Burr, C. T. Lane, and L. F. Nims, A vacuum tube microvoltmeter for the measurement of bioelectric phenomena. *Yale Jour. Biol. and Med.* **9**: 65 ff., 1936.

[4] When a very small surface of contact is desired, it may not be the best procedure to construct a correspondingly narrow non-polarizable electrode. The same purpose is served if the crucial area of the scalp is covered with collodion, while the end of a narrow rod is pressed against the scalp in the middle of this area. If now an electrode of ordinary size is attached to the area, contact of the salt solution with the scalp is restricted to a surface equal to the cross-section of the rod.

The rhythm is also blocked when the *S* merely opens his eyes. Our next record demonstrates this fact (Record 2). When it was taken, the experimental room was almost entirely dark; but, as the curve shows, the waves disappeared a short time after the eyes had been opened (*cf.* the mark *O* on top). The rhythm was reestablished when the eyes were closed again (mark *C*).

Records 1 and 2 very closely resemble those taken of the same facts with other instruments. Since we obtained blocking of the α-rhythm also when an auditory stimulus was applied, we can safely conclude that our instruments give a reliable account of brain events which do not involve very high rates of change.

Several observations which we repeatedly made may not be directly related to our main problem; but one must be familiar with these facts if misinterpretations of records are to be avoided. In the



RECORD 2. Subject O'C. The α-rhythm is temporarily blocked by opening of the eyes.

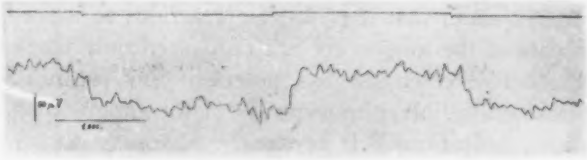
first place, while opening of the eyes is usually accompanied by a slight deflection downward which tends to persist (*cf.* Record 2), a blink, that is a momentary closing of the eyes, causes a short deflection upward. With the connections used in our circuit, a deflection downward corresponds to a positive potential under the active electrode.

Secondly, even when the electrodes are both attached to the back of the head, eye movements up and down or to the right and to the left also cause deflections. These responses may be registered over and over again for considerable periods. Because of their great regularity, one feels inclined to assume that the origin of such responses is located in the periphery of the head, including the eyes, rather than in the brain. The reactions do not seem to accompany eye movements as such; for, in our records, the deflections persist so long as the eyes remain turned one way or another. Records 3 and 4 may serve as examples. Both

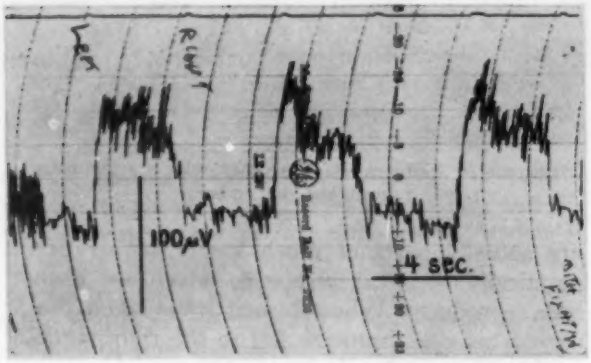
RECORD 3. Subject P. Currents which accompany lateral eye movements.

are actually parts of much longer sequences of practically identical responses which we cannot here reproduce. When Record 3 was taken, the *S* moved his eyes from the left to the right, and so forth. The angle of the movement was almost maximal. Both electrodes were attached over the region of the cortical fovea, one on the left, and the other on the right side of the median plane, the one on the right side being active. When the eyes move only from a fixation mark straight before the *S* to an extreme position on one side, the deflection is about half as large as when the movement occurs from one extreme deviation to the other. Record 4 shows the responses which accompany deviations of the eyes up and down, beginning with a movement downward. The sizes of the deflections in Records 3 and 4 cannot be directly compared. When Record 4 was taken, the active electrode was placed over the region of the cortical fovea, and the grounded electrode on the vertex, which means that their distance was much greater in the situation to which Record 4 refers.

It is, of course, important to know that under the conditions of our experiments eye movements cause deflections of the base line, because the eyes tend to move when objects are shown in peripheral positions. But the electric responses which accompany eye movements are subject to certain restrictions. When the line connecting the electrodes has a transverse direction with regard to the median plane, movements to the left and to the right are readily registered, but movements up and down have only a very faint effect on the base line. Conversely, when one electrode is placed behind the other, eye movements up and down give strong responses, while movements to the



RECORD 4. Subject P. Currents which accompany eye movements up and down.



RECORD 5. Subject K. Currents which accompany lateral movements of the tongue.

right or to the left cause no deflection, excepting perhaps a weak reaction which is limited to the time of the movement as such. This simple rule must be remembered in the discussion of records taken under conditions of visual stimulation.

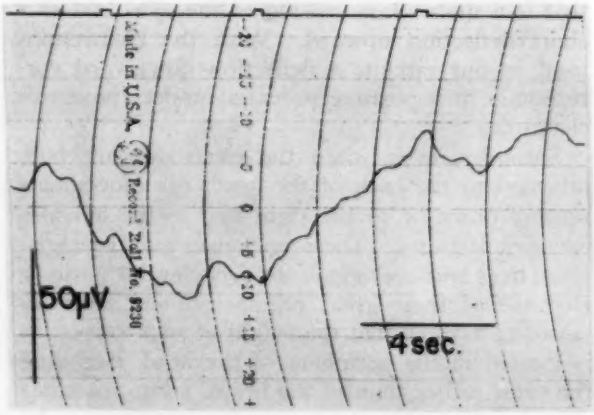
So far as we can see, our records of eye movements agree entirely with those of other experimenters.[4a]

As a third fact with which experimenters must be acquainted, we have to mention the electric effects of movements of the tongue. With the tongue protruding from the mouth, but not only in this situation, any major movement which the tongue performs to the right or to the left is accompanied by a deviation of the base line. The deviation appears to persist as long as the lateral position of the tongue is maintained (Record 5). The present record was taken with the active electrode in a place perhaps corresponding to Brodmann's left area 41, and with the second electrode attached to the vertex. When the active electrode is placed in the corresponding position on the other side of the head, while the second electrode remains on the vertex, the polarity of the responses in their relation to the direction of the movements is reversed. In such situations, the distance of the active electrode from the representation of the tongue in the sensorimotor region of the cortex cannot be very great; the registered potentials might therefore issue from this region. But we do not claim that they do; for reactions to movements of the tongue are not registered only when the active electrode has precisely this location. Incidentally, other movements of the tongue also cause deflections. It is clearly necessary to instruct *S*s to keep their tongues quiet when records are not meant to refer to such movements.

We have not tried to clarify the nature of these responses, or of those which go with deviations of the eyes. We mention them only because responses of this kind might also appear in records which are supposed to demonstrate other phenomena.

II. VISUAL CURRENTS

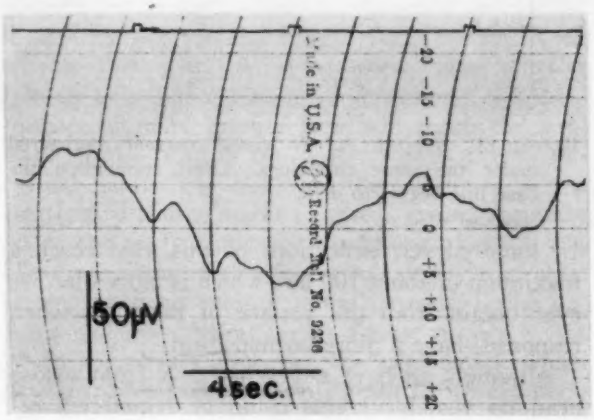
In our first attempts to register currents which issue from the striate areas during visual perception, we used the following experimental situation: While the *S* fixated a faint mark in the middle of the field, a bright object appeared in the left or right periphery, slowly approached the fixation mark, passed it, and then moved toward the opposite periphery where it disappeared. The active electrode was placed approximately in the middle of the region under which the foveal parts of the visual cortex are located, and the second electrode was attached to the vertex. Three records obtained under these conditions have been reproduced elsewhere (13, Records III–V). We now show seven further curves, taken from the heads of four different *S*s, which refer to the same situation (Records 6 to 12). All exhibit responses to the moving object. (The three marks on top correspond to the first appearance of the object, to the moment at which it passes the fixation mark, and to its disappearance.) Although the particular form of the responses varies considerably from one instance to another, all deflections have the downward direction. It is thus confirmed that, in relation to the vertex, a



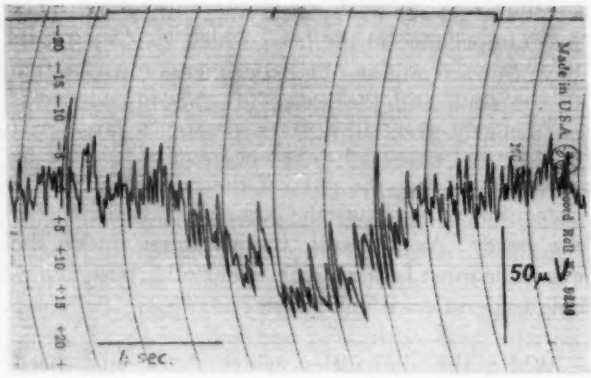
RECORD 6–12. Subjects M., R., A., and B. Visual currents registered while a bright object moves across the field. The active electrode is placed in the median plane.

[4a] *Cf.* L. Carmichael and W. Dearborn, Reading and visual fatigue, ch. 6., Houghton Mifflin Co., 1947.

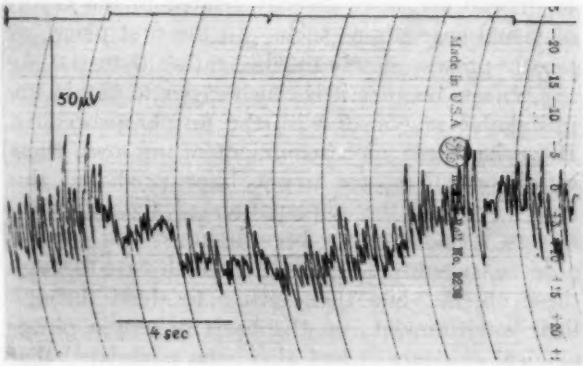
positive potential develops under the occipital electrode when a bright object appears in the visual field. If the registered currents actually issue from the visual cortex, the potential at the surface of the head must be derived from a much higher positive potential at the surface of the brain. Immediately under the electrodes, the difference of potential at maximal excursion varies in these records from about 25 to 55 μV.



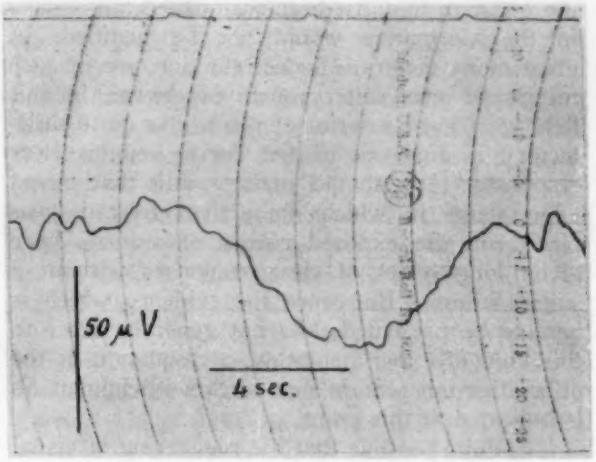
RECORD 7.



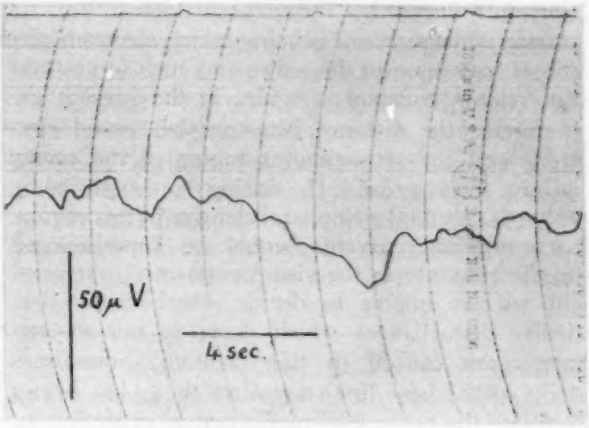
RECORD 8.



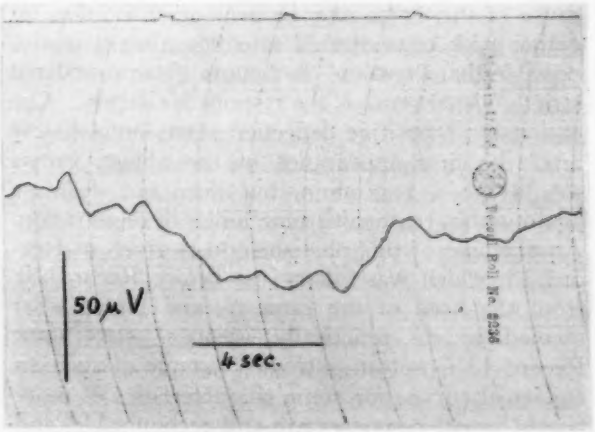
RECORD 9.



RECORD 10.



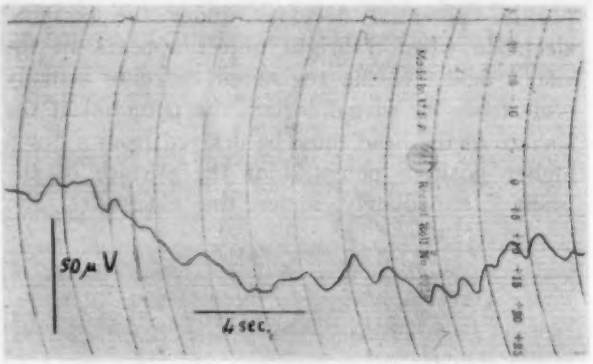
RECORD 11.



RECORD 12.

Would results such as these be obtained whenever electrodes are placed in the same positions, and slowly moving objects are presented? As the question has just been formulated, an answer in the affirmative would not be justified. A great many factors influence the outcome of such tests, and even after much experience in this field we regard a series of records as quite satisfactory if nine out of ten curves exhibit clear responses. We should perhaps add that recent experiments in which the active electrode was placed on the exposed cortex of animals have given long series of clear responses without a single failure. But since the evidence which is here to be presented does not generally refer to this kind of experimentation, a discussion of the difficulties inherent in our work with human *S*s is indicated at this point.

It is quite possible that the registering of visual currents is sometimes disturbed by influences entirely extraneous to the conditions of stimulation introduced by the experimenter. For instance, while a record is being taken, electromotive forces may arise in the scalp, and such forces may not remain constant. Again, in the present experiments the distance between the second electrode and the sensorimotor region of the cortex is not very great. If, during an experiment, voltages should develop, and change, in this region, corresponding currents would be superimposed on the response to the visual exposure. At present we are unable to decide whether any particular disturbances which occur in our experiments are caused in this fashion. Sometimes drifts of the base line start after the *S* has begun to fixate the prescribed mark, and is expecting an exposure. Such drifts may occur when the electrodes are in perfect working condition. The cause of the drifts may be prolonged fixation together with concentrated attention; but it is also possible that "tension" in a more general and not strictly visual sense is the responsible factor. Occasionally, a positive deflection starts immediately after the first appearance of the object, grows steadily to a maximum, but does not decrease again within reasonable time limits of registration. An example of this phenomenon is given in Record 13, which was taken just before Record 10, from the head of the same *S*, and under what seemed to us practically identical conditions. Record 13 is not an extreme example of such an abnormal curve; for some characteristics of more typical visual responses are still recognizable, and the size of the deflection is by no means unusual.



RECORD 13. Subject A. A visual current registered under the same conditions. Upon stimulation the base line begins to drift.

In some curves, deflections of this kind reach a maximum of about 100 μV, which is more than we ever obtain from the surface of the head when responses have a more normal form.

Although we have never had a *S* from whose head no visual currents could be registered, responses are clearly weaker with some individuals than with others. The difference may be related to simple anatomical variations among *S*s. The main impedance in the head which visual currents have to overcome in our experiments must be that of the skull and the scalp. If this impedance is particularly great in a given person, a large part of the total potential difference available must be spent in driving the part of the current which is accessible to us from the brain to the surface of the head. As a result, the potential under the electrode must be low, even under otherwise favorable experimental conditions.

While the difficulties which have just been mentioned may be regarded as mere technical obstacles, other factors on which results have proved to depend are more directly related to the nature of visual currents as such. In the first place, we mostly present slowly moving rather than stationary objects because it is much easier to obtain unmistakable responses with the former procedure. If we had been more confident of our own theoretical premises, we might have predicted this fact. For, in the explanation of figural aftereffects, it had not only been assumed that cortical objects are sources of currents which pass through these objects, and then return to them through their environment; on the basis of ample physiological evidence it had also been postulated that such currents weaken themselves by their own

electrotonic action. If it was true that, in vision, a considerable degree of this obstruction develops within a very short time, then it followed that the registering of the currents of stationary visual objects must be a fairly difficult task; because such currents continue to flow in a given part of the medium, and can thus raise the obstruction to ever higher levels. It was obvious that, even though electrotonic action begins at once, its obstructing effect must be less pronounced if the source occupies given cortical places only for moments, and that, as a consequence, a more intense flow will reach the surface of the head if the object moves across the visual field. The objection that, at first, even a stationary source ought to give a strong response does not apply under the conditions of our work. Our recording device reacts too slowly for registering such an intense initial current, if the decrease of this current begins almost at once.

In actual fact, however, the explanation which has just been given was only gradually forced upon us when one observation after another indicated that one cannot investigate the visual currents of the brain without being continually confronted with their electrotonic effects. We shall presently see that, even when the object moves, some such effects are probably always present.

A second factor which strongly influences results is the physical brightness of the object or, more probably, the relation between this brightness and that of the background. Contrary to what one might expect, good responses are most readily obtained when only moderate brightness differences are used. In this respect, the experimental condition which would at first appear to be most promising has proved to be quite unfavorable. A very great brightness difference can be established by projection of a narrow strongly illuminated slit on a screen in an otherwise dark room. In our experiments, results have rarely been satisfactory with this procedure, even when the bright object moved; and it now seems clear that, in the beginning of our work, convincing responses occurred only when, more or less by accident, there was some additional illumination in the room, which reduced the brightness of the projection in its relation to that of surrounding parts. This may appear to be a paradoxical finding since, according to our theoretical assumptions, the electromotive force which drives the current through the tissue must grow when the brightness difference between the object and its environment is increased. Actually, intensification of this force need not cause stronger responses under all circumstances. Again, the electrotonic action of the visual current must be considered. This action probably explains the present paradox also. The main point is that the current of a cortical object is electrotonically active not only in the interior of this object but also in the surrounding medium. It is true that, on the average, the density of the flow must be greater on its way through the circumscribed area of the object than outside, where the same total current spreads more widely on its way back to the object. But the density in parts adjacent to the object must nevertheless be considerable, for here the lines of flow form very short curves around the boundary of the object, which means a low resistance and a relatively high density of the current along these lines. As a result, in such adjacent parts electrotonic action must still be fairly intense. In one direction, however, these are the parts in which the object will later be located on its course across the field. Thus, as the object approaches a certain region, it partially blocks the way of its current through this region "in advance." It seems quite possible that, with very great brightness differences, so much obstruction accumulates in this fashion by summation in time that during later parts of the exposure the current is too weak to establish effective potentials on the surface of the head. Our instruments, we repeat, could not register the very short flow of great intensity with which this process would begin.

Very small brightness differences give, of course, weak potentials. With such differences, one can therefore not expect to obtain striking records, quite apart from any electrotonic obstruction which might arise under the circumstances. Consequently, there ought to be a range of moderate brightness differences within which responses are optimal. According to our experience, this is a correct conclusion. The optimal region seems to correspond to a fairly low ratio of the intensities within the object and outside. Often, responses have been particularly impressive with intensity ratios surely not higher than 5 or 10 to 1, and occasionally we have obtained recognizable deflections when the brightness difference was not very much greater than its threshold value in a psychological sense. No quantitative investigation has so far been done in this respect because results depend also upon many other factors, which we have not been able to keep strictly constant in our experiments.

The fact that the optimal brightness ratio is by no means high is most convincingly revealed in experiments in which the physical object and its background are both surfaces seen in the diffuse light of a lamp. It is well known that intensity ratios are always rather low under these conditions, even though the object may look white, and the background surface black. We discovered by accident that visual currents can be registered under such circumstances, when we tried to take measures against major changes of retinal adaptation which might occur during the exposure of the bright moving object. Such changes could best be prevented if the general intensity level in the *S*'s field before exposures was not much lower than that during exposures, and so we cautiously introduced stronger permanent illuminations of the experimental room. At the time, a luminous rod was used which moved in front of a dark surface. The intensity of this source was varied in order to find optimal conditions. When there were responses even with the lowest intensities, it became obvious that at this stage the crucial brightness difference was no longer that between the light of the rod and the dark background, but rather that between the bright surface of the rod as such and this background, both seen in the general illumination of the room. Actually, responses then proved to be just as good when the source emitted no light whatsoever. We have taken numerous records under such conditions, mostly with a strip of white or grey cardboard, which moved across the field before a large piece of black cloth. The response shown in Record 12 was obtained with this procedure. The object was a tall white cardboard rectangle, the background a black screen, and the whole situation was strongly illuminated from the ceiling by a powerful lamp such as one finds in lecture rooms. Further records taken with "real" objects rather than with bright projections in weakly illuminated rooms will be shown in connection with other experiments. Incidentally, out of the 11 records reproduced in an earlier article (13), 8 were registered under such conditions. The fact that it is not at all difficult to obtain responses in such situations is important also from a more general point of view. Occasionally, discoveries are made under special laboratory conditions, which cannot easily be repeated in a more natural environment. Our results are not of this kind. We do have to use instruments, and we must attach electrodes to a *S*'s head; but any white object that moves, for instance, before a dark curtain in a living room would surely also give a response.[5]

On the other hand, when records are taken while a real object moves across the field in a fairly strong illumination, one is sometimes confronted with a difficulty which can more readily be avoided in experiments with projected objects. In the former situation, stationary contours are likely to appear in peripheral vision, which may influence records by their protracted electrotonic action.

It is our impression that if the area of the moving object is very small the registered currents tend to be weak. Theoretically, one would expect such an influence. The density of the current within the object and in adjacent parts must grow when its size is reduced, and therefore electrotonic effects must here be accelerated under the circumstances. As a result, the same difficulty is likely to arise as is caused by very great brightness differences between the object and its background.[6]

We have sometimes used moving objects of considerable width. For example, when Records 8 and 9 were taken, a projected vertical rectangle with the width 10 cm. moved across the screen. Since the distance of the screen from the *S*'s eyes was 50 cm., this corresponds to a visual angle of about 11° in the middle of the screen. Generally, it is true, the size of our objects was considerably smaller. Thus the strip of cardboard used when Record 12 was taken had a width of only slightly more than one degree.

From a practical point of view, it is a most inconvenient fact that, when records are taken one after another without sufficient rest periods, results soon tend to be disappointing. We very much suspect that this happens because the electrotonic action established during one exposure still persists when the next test is given, and so forth. One is tempted to do many experiments in a rapid sequence, either because he is impatiently trying to find optimal experimental conditions, or because several records have just been particularly satisfactory. The experimenter

[5] Recent observations make us suspect that while the size of the brightness difference between the object and its environment is an important factor, the absolute brightness level used in such experiments also influences results. Good responses seem to be most readily obtained when the average light intensity in the field is fairly high.

[6] While the currents of wide objects are less dense than those of narrow objects, their electrotonic action affects a given place for a longer time. Apparently, these two factors do not cancel each other.

should not yield to this temptation. We have often found that after very long rest-periods results tend to be particularly clear. It is not a very bold assumption that the detrimental influence of very frequent exposures is caused by accumulation of electrotonic obstructions. The investigation of figural after-effects has clearly shown that even weak effects of this kind persist for considerable periods. Direct physiological tests which refer to this question are, of course, also possible. A surprising consequence of often repeated experimentation with given *S*s will be mentioned later.

The retinal speed with which the object moves has been varied a great deal in our work, at times by changes of the velocity of the physical object, but often also by changes of its distance from the *S*. Speeds beyond a certain rate must be avoided because, if they were used, the *S*'s eyes might be forced to follow. With extremely low speeds, on the other hand, the same difficulties would arise as make it hard to obtain good records when the object is stationary. We have used retinal speeds between about 3° and 12° of visual angle per sec. Other conditions being strictly constant, variations within this range may prove to have an influence upon results, if merely because electrotonic action is affected by the speed of the cortical object. But we have no evidence that within the range which has actually been used by us certain speeds are to be preferred to others. Records 6 to 12 were taken with retinal speeds between 5° and about 10° of visual angle. The total angle of the movement also varied. In these experiments it amounted to about 60°. In other tests, it has been as large as 110°, and as small as 15°.

The direction in which the object moves does not seem to influence the form of the responses. On the average, records taken with the object moving from the right side to the left do not differ from those in which the direction is reversed. A more detailed investigation may nevertheless show that in individual cases registered responses are stronger when the object moves in one-half of the field, say, the right half, than in the other. Mere asymmetries in the anatomy of the skull on both sides of the median plane might cause such differences. If they occur, they have so far been obscured in our work by stronger varying influences. Recently, all our records have been taken when the object moved in an horizontal direction. But we have also obtained results with a vertical movement of an object shown in an horizontal orientation.

In the course of this investigation, we have learned to give a great deal of attention to the position of the occipital electrode. Occasionally, it happened that a *S* whose records had always been particularly satisfactory gave only very weak responses when the active electrode had just been attached for a next experimental session. In several instances, it was then discovered that the electrode was not in the right position. An error as small as 1 cm., say, in the upward direction, may reduce the size of the responses considerably. This observation is, of course, important not only from a practical point of view but also because of its implications as to the origin of the currents.

We can now discuss Records 6 to 12 in more detail. The form of the responses in these records is obviously a consequence of the way in which the object is presented. There can be no question that the active electrode is placed in a position quite near the cortical representation of the fovea. A stronger current must therefore flow into the leads when the object approximately coincides with the fixation point (second mark on top), while on both sides, toward the periphery, the responses must be weaker. With one curious exception (Record 11), the actual shape of the responses in the present records agrees with this conclusion, and hence with a well-known fact in brain localization (*cf.* also 13, Records III–V).

It follows that variations in the form of the exposure must influence the shape of the responses. For instance, when the object suddenly appears in the middle of the field, and then moves (or expands) toward the periphery, responses must be maximal almost immediately, and must then decrease. Elsewhere we have shown curves which confirm this expectation (7, Records VIII–XI). We once hoped that the influence which changing positions of the object have upon the shape of the response could be eliminated by showing a line in the periphery which points at the fixation mark, and rotates around this mark. We had no difficulties in obtaining results under these circumstances; but when the line was not too far removed from the fixation mark to give appreciable potentials, a complication arose which we were not able to overcome. In turning around the fixation mark, the line did not change its distance from this mark. As a result, the cortical region toward which it pointed was subjected to constant electrotonic action. Accordingly, the form of

the responses was by no means simple; it generally resembled the complicated shapes which are sometimes found when stationary objects are presented.

The greatest deflection does not always coincide precisely with the fixation mark. In the present records, there is occasionally no sharply defined maximum, sometimes the maximum lies at a point where the object must just have passed the mark, and once it occurs before this mark. In many other cases, the greatest deflection does coincide with the moment at which the fixation mark is reached (*cf.*, for instance, 13, Records IV and V). We are not yet able to explain the variations. The delay of the maximum repeatedly found in the present records is too great to be caused by slow reaction of the recorder, since such a delay is also obvious in Records 8 and 9, which were taken with our fast galvanometer. The strikingly asymmetrical position of the greatest deflection in Record 11 is fairly unusual, and most probably the effect of a not entirely satisfactory experimental technique. The record is here shown precisely for the reason that it exhibits a great deviation from more normal responses.

Some further remarks about the specific characteristics of Records 6 to 12 are indicated. Records 6 and 7 were taken from the same subject, during the same experimental session, under identical conditions of presentation, but separated from each other by several other tests. Closer inspection will show that, apart from the different location of the greatest deflection, these records agree even in details. On the whole, the size of the responses is greater while the object moves in the first half of the field, a fact to which we shall presently return. In both curves, the main response is followed by a second deflection, which has the same polarity. This second deflection occurs in a great many records, so often, in fact, that it cannot possibly be regarded as a random fluctuation of the base line. Without any question, this is an after-potential.

Another example of an after-potential will be found in Record 13, in which the response failed to return to the original level at the end of the exposure. At this point, the size of the response is appreciably reduced for a moment; but then a second positive deflection follows just as it does in Records 6 and 7.

Records 8 and 9 were also taken from one *S*, and during the same experimental session.[7] The faster galvanometer was used in the recorder, and as a consequence the curves are covered with rapid fluctuations.[8] The two records differ from each other in that the response in Record 8 does not begin until the object has moved across a considerable part of the field. Similarly, the response is already quite weak some time before the exposure ends. Another record from the same *S*, which has been shown elsewhere (13, Record V), exhibits the same peculiarity. In Record 9, on the other hand, the deflection begins immediately after the appearance of the object in the periphery. This variation occurs frequently; we do not know for what reasons.

Records 10 and 11, taken in this sequence with a third *S*, one immediately after the other, show a particularly striking difference as to the size and the form of the responses. We suspect that the relative weakness and the distorted shape of Record 11, to which we have referred before, have been caused by the fact that the *S* was given no rest period before the record was taken. Incidentally, in both records weak after-potentials again follow the main response.

In Record 12, a plateau rather than a more circumscribed maximum appears in the region of the fixation mark. The second positive deflection after the end of the exposure may be a weak after-potential. This is the record taken when a piece of cardboard served as object, and when the room was very strongly illuminated.

The fluctuations which cover the responses in Records 8 and 9 deserve particular attention. The waves are those of the α-rhythm. They are not very regular in any parts of Record 8, but quite regular in Record 9 before the exposure begins, and do not degenerate in this curve until the object has just appeared in the periphery. At this time, the deflection corresponding to the visual current has also just begun. When the recording device operates fast enough so that the rhythm can be registered, curves often demonstrate the following facts which suggest a certain interpretation of the blocking of its waves. In the first place, if a *S*'s rhythm is at all conspicuous, a more or less constant visual situation does not really suppress it for any great length of time. This holds at least so long as the average stimulation of the eyes is not very strong.[9] Apparently, the α-rhythm becomes adapted to varying

[7] During these trials, the fixation mark was by accident not placed precisely in the middle of the exposure field.

[8] In the following, it will no longer be mentioned in each case whether a given record was taken with the faster or the slower galvanometer. The curves themselves sufficiently indicate the difference.

[9] We do not wish to deny that darkness is a condition in which the rhythm is particularly strong.

levels of stimulation, although this process takes some time. But the appearance of an additional bright object in the field does disturb the rhythm within a fraction of a second. Both phenomena are clearly shown in Record 9. There is adaptation inasmuch as, early in the record, the *S*, who has been in a more or less constant visual situation for a considerable period, fixates the prescribed mark in expectation of the exposure, his eyes being exposed to a considerable amount of light. Obviously, this does not disturb his α-rhythm very much. (It seems that the *S* is not yet adapted to the same degree while he waits for the exposure in Record 8.) Secondly, however, the rhythm shown in the beginning of Record 9 is greatly affected a very short time after the bright object has entered the field. Only during the second half of the exposure has adaptation again advanced so far that the amplitude and the regularity of the waves are almost the same as they were before the exposure.

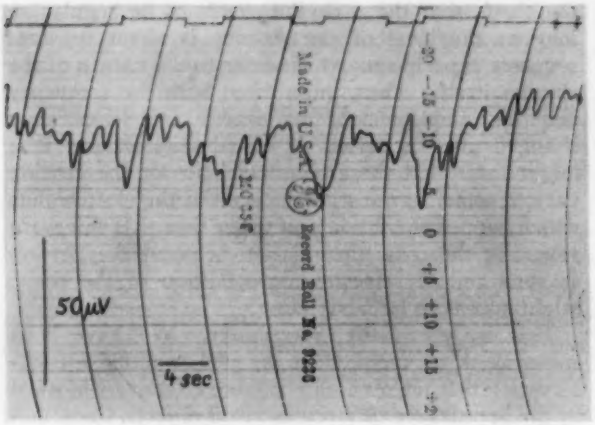
It is an interesting observation that in Record 9, but by no means only in this instance, the rhythm is profoundly affected just when the appearance of a new bright object has caused a current to flow in the visual cortex, or has intensified currents which have been present before. If we are right in saying that a new object invariably causes such a current, whether the object is the first to appear in a former homogeneous field, or is added to others, then the coincidence to which we have just referred is a perfectly general fact: disturbance of the α-rhythm occurs when new currents have just begun to flow in the visual cortex. Under the circumstances, there is a possibility that when the α-rhythm is blocked, this happens *because* cortical currents have just begun to flow. In what fashion would such currents affect the rhythm? Blocking of the rhythm always occurs a short time after stimulation of the eyes has begun, or after it has been increased, which means, in terms of the present argument, when the new current has flown for a short period. But it always takes the current a short time to establish a certain amount of its electrotonic effect. It would therefore seem to be a reasonable assumption that new currents disturb the α-rhythm by raising the level of electrotonus, and that for this reason the disturbance does not occur at once. The fact that, after a while, the rhythm is adapted to the new condition would have to be interpreted as meaning that regular α-waves can occur whenever the electrotonic condition of the tissue is more or less constant. In a given situation, however, that condition will always tend to become fairly constant because electrotonic action is a self-limiting process. The stronger the electrotonic effect grows, the more will the currents be weakened which cause the effect.

It should not be particularly difficult to subject this theoretical possibility to experimental tests. Also, the thesis that the α-rhythm tends to be regular so long as the level of electrotonus is about constant suggests experiments which refer to the nature of the rhythm itself. Thus, quite often both the frequency and the amplitude of its waves are temporarily changed after a period of visual stimulation. One might infer that these characteristics of the rhythm vary to some degree with the level of the electrotonus which happens to obtain at a given time. If this were generally the case, the relation between the rhythm as such and the electrotonic condition of the tissue might appear to be fairly close.

This is, admittedly, speculation. We have been encouraged to mention it by the well-known observation that the α-rhythm is more effectively blocked by the appearance of circumscribed objects, lines, and the like, than by a bright homogeneous field (1). A homogeneous visual field may establish a current with the whole visual cortex as a source; but, as we remarked once before, the density of such a current would be low, much lower than that of a current which passes through cortical objects of limited area. As a result, the electrotonic action of the latter would be much more intense, and a more effective blocking of the rhythm would follow.

Is it a serious objection that the rhythm registered from occipital parts of the head may be blocked not only by visual stimulation but also by sounds, other sense impressions, the focussing of attention on an object, and so forth? One cannot yet tell. No difficulty would arise if such facts were also accompanied by currents, and if such currents spread far enough to affect the visual cortex by electrotonic action. It would, of course, be quite possible that they do, even if, with the electrodes placed as they are in our experiments, the currents which issue, say, from the auditory cortex cannot as such be registered from the surface of the *S*'s head. Apart from this possibility, there are also other ways in which non-visual processes may affect the α-rhythm of the striate area.

We will now report about experiments which are simple variations of those just described. Since responses are not always satisfactory when electrodes are placed on the scalp of human *S*s, we have repeatedly taken records which serve to demonstrate the reliability of our findings once favorable experimental conditions have been established. For this purpose, regular responses to a series of exposures rather than to a single presentation of the moving object may be registered. The series must, of course, be rather short, because otherwise responses later in the series would be too strongly affected by obstructions established during earlier exposures. Four records of this kind have been reproduced in one previous article (7, Records I–IV), and two in



RECORD 14. Subject S. Responses to four exposures of a moving object.

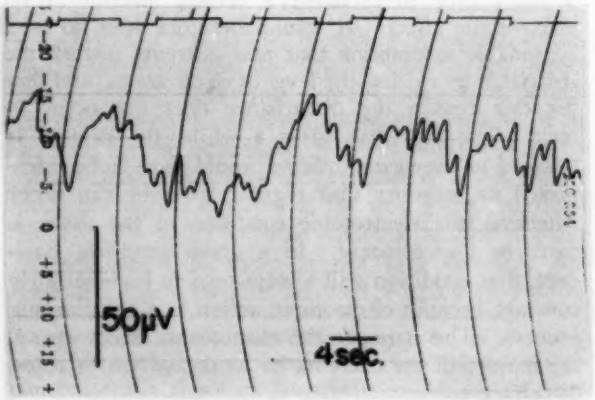
another (13, Records I and II). It will therefore not be necessary to show many further examples. When inspecting Record 14, one has to realize that both the speed of the recording paper and the length of the individual exposures are now considerably reduced. In this fashion, the total length of the record is kept within reasonable limits. The projected object moved four times in succession with a retinal speed of about 7° per sec. across a visual angle of 37.5°. The time from the beginning of the first to the end of the last exposure was half a minute. Between exposures, only the mark which the *S* fixated without interruption remained in the field. Four responses, all of the same polarity as those shown in Records 6 to 12, are clearly discernible in the present curve. The maximal deflection coincides in each case with the middle of the corresponding exposure period as marked on top, i.e., with the moment at which the object passes the fixation mark. The deflections have, of course, the form of short waves only as a result of the technical measures just mentioned. The oscillations which are superimposed on the whole record represent this *S*'s slow, steady, and very strong pulse. Records taken from his head tend to show his pulse wherever the electrodes are placed.

We add one more record taken under similar circumstances from the same *S*. Record 15 gives the responses to five exposures presented in one experiment. There seem to be only four responses, however; those to the second and third exposures have fused into one. When trying to explain this fact, one cannot assume that the visual current accompanying the second exposure persists, as such, until the following current has begun to flow. The interval between the end of the second and the beginning of the third exposure amounts to 3 sec. We have no evidence that visual currents which accompany the exposure of a moving object ever continue to flow for such a long time after the object has disappeared. A better explanation can be given if it is remembered that, when the flow of visual currents is just disappearing, records often exhibit secondary deflections or after-potentials, which have the same polarity as the preceding visual currents, and may be quite strong. In the present case, such an after-potential probably fills the gap which would otherwise have occurred between the second and the third responses. It happens that this interpretation is supported by the appearance of an obvious after-potential after the fifth exposure of the series. This after-potential has almost the size of the preceding visual responses.

III. LOCALIZATION

In records taken with the active electrode placed on the median plane, the response is generally maximal more or less at the time when the object passes the fixation mark. Under the circumstances, the position of the electrode is not likely to favor a stronger response either in the right or in the left half of the visual field, although individual responses may have a somewhat asymmetrical shape as a consequence of anatomical conditions, or for other reasons which will presently be considered.

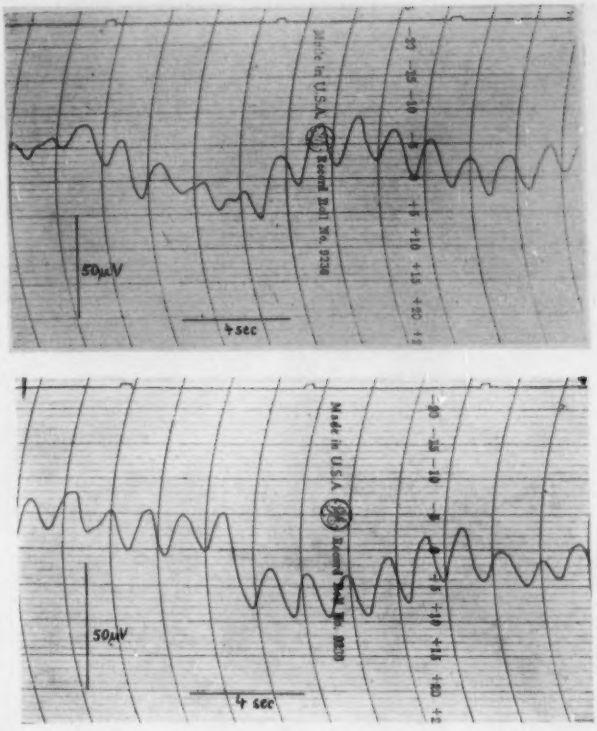
More regular displacements of the maximal deflection and corresponding asymmetries in general are to be expected if the active electrode is given a position on one side or the other of the median plane. For instance, with the electrode on the



RECORD 15. Subject S. Responses to five exposures of a moving object. Two responses have fused.

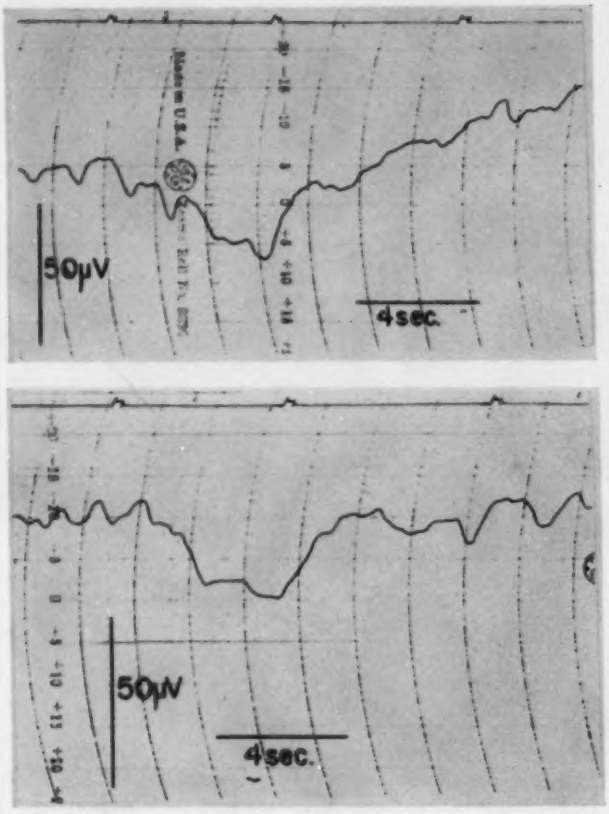
left side of this plane, the cortical object ought to raise the potential at the electrode more strongly while the object moves in the right half of the visual field; and the opposite should happen if the electrode is placed on the right side. To be sure, measured in an horizontal direction, the visual tissue located directly under the skull is, in the case of man, only a few centimeters wide. But this may just suffice for experiments in which the active electrode is to be given a lateral position. In our present argument, the situation would, of course, be over-simplified, if it were true that the whole fovea is represented both in the right and the left striate areas. In the absence of decisive data in this respect, we could, of course, do no more than compare the results of actual tests with the expectations derived from the argument. Four records which have been shown elsewhere (13, Records VI–IX) agree entirely with the expectation, and so do three others reproduced in a further article (7, Records V–VII). The same is true of our next two records (16 and 17), which were taken from the same *S*, one very soon after the other and, with one essential exception, under the same conditions. The object was a vertical strip of bright cardboard which moved horizontally in a fairly well illuminated room before a dark background. The active electrode was attached at the usual height, but on the right side of the head, about 1.5 cm. from the median plane; the second electrode remained in its usual position on the vertex. We assumed that under these circumstances a more intense current would flow through the leads when the object moved in the left half of the visual field, and that, therefore, the registered response would be stronger during the first half of the exposure when the object moved from the left to the right side. With the direction of the movement reversed, the opposite asymmetry in terms of time was expected. Record 16 shows the result for the direction left-right, Record 17 that for the opposite direction. The responses, on which the *S*'s pulse is again superimposed, obviously agree with our predictions. Other records have shown that, when the active electrode is placed on the left side of the median plane, asymmetries in time also confirm the predictions which must be made for this situation. The behavior of the currents registered in such tests is, therefore, the one which agrees with the more widely accepted views concerning localization within the visual cortex.

In Records 16 and 17, deflections have approximately the same size whether they occur mainly



RECORDS 16 and 17. Subject S. Responses to a moving object when the occipital electrode is placed above the right foveal cortex. In Record 16 the object moves from the left to the right, in Record 17 from the right to the left.

in the first or in the second half of the exposures. It does not seem to matter whether, temporally, it is the first or the second half in which, according to predictions, the response must be stronger. Hence, both curves appear to give equally convincing evidence of localization as tested when the active electrode has a lateral position. Actually, any experimenter who works in this field will soon discover that, in this respect, Record 17 is far more important that Record 16. When the two forms of the present test are often repeated, results leave no doubt as to a strong influence which the sequence of the two halves of an exposure tends to have upon the shape of the curves. It is much easier to obtain the expected asymmetry when, according to our premises, the stronger deflection must occur in the first half than it is when the response must be stronger in the second half. While this general statement is amply justified by our findings with the electrode in a lateral position, it also agrees with the observation that, even when the electrode is placed in the median plane, the second half of the response is often



RECORDS 18 and 19. Subject An. Records taken under the same conditions as Records 6–12. The second half of the responses is weaker.

weaker than the first. We have called attention to this fact when discussing Records 6 and 7, where this effect of sequence in time was fairly obvious, even though in Record 7 the very greatest deflection happened to lie in the second half. Sometimes the effect is so extreme that the response almost disappears when the cortical object has just passed from one hemisphere into the other. The next records (18 and 19), taken on the same day from the same subject, and with the electrode in the median plane, illustrate this point very strikingly. The early parts of the responses are quite similar to those shown in Records 6 and 7, with which they are comparable so far as the main experimental conditions are concerned. In both instances, the current begins to flow soon after the object has appeared in the periphery, and its registered intensity grows to a maximum as the object reaches the fixation mark; but afterwards the response decreases so rapidly that only a weak deflection is left in Record 18, and practically none in Record 19. Now, when Record 18 was taken, the object moved from the right to the left side, while the exposure for Record 19 was given in the sequence left-right. The half of the field in which the response almost, or entirely, disappears is therefore the left half in Record 18, and the right half in Record 19. It is, of course, in both curves the second half in terms of time. It follows that the extreme asymmetry of these responses cannot be explained by anatomical or functional differences between the two hemispheres, or by an asymmetry in the impedance of the skull and the scalp on both sides of the median plane. It can also not be attributed to an error as to the location of the electrode in relation to the two parts of the visual cortex. Plainly, the second half of the response tends to be considerably weaker only because it is preceded by the first half. Although this phenomenon is not equally pronounced with all *S*s, it occurs quite often with most, and we could show curves obtained from some which are as striking in this respect as Records 18 and 19. With the *S* from whom the latter records were taken, we once did eleven such experiments, under the same conditions, and all in one session. Five times the object moved from the left to the right, and six times from the right to the left. In no instance was the response stronger in the second half of the exposure; nine times it was clearly stronger in the first, and twice the shape of the deflection could be regarded as symmetrical.

Since this can happen with the electrode placed in the median plane, experiments in which it is placed on one side must, obviously, be interpreted with great caution. To be sure, according to our premises, responses must be stronger during the first half of the exposure when this half corresponds to the side on which the electrode is placed. But, if they are, this fact alone does not yet demonstrate an agreement with cortical localization; because in the sequence of the two halves the first is at a certain advantage under all circumstances. On the other hand, when the second half of the exposure corresponds to the location of the electrode, a stronger response in this half can, of course, be trusted *a fortiori*. For this reason, we refrain from showing further examples of the former kind, and will add only two more instances of the kind which can be regarded as conclusive (Records 20 and 21). In both instances, the active electrode was placed on the left side of the median plane, and the object moved from the left to the right side of the field. The responses shown in these records are clearly

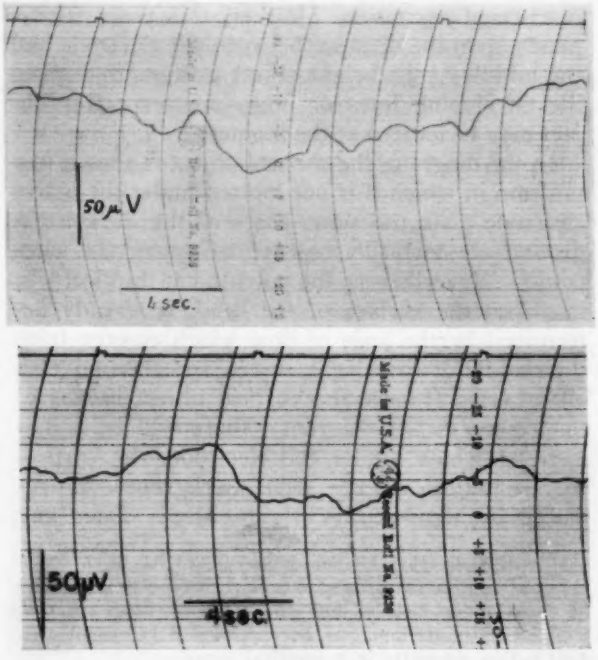
stronger during the second half of the exposure, as they are expected to be under such conditions.

Incidentally, even satisfactory records of this kind mostly exhibit what might be called a latent sequence effect. Currents are actually stronger during the second half of the exposures; but the difference between the two halves tends to be smaller than it is when the first half of the response must be expected to be stronger, and is then found to be so.

There seems to be only one way in which the first half of a response can have an influence on its second half, a considerable part of which occurs several seconds later. The current which flows during the first half must affect the medium in which the cortical object will then move during the second half. Moreover, since the response in the second half is weakened, the alteration of the medium which the current here establishes during the first half must have the character of an obstruction. It thus becomes an almost necessary conclusion that, when a cortical object moves in the striate area of one hemisphere, its current is electrotonically active not only in this hemisphere, but also in the striate area of the other. In an earlier discussion we considered the concept of electrotonic action "in advance" precisely for this reason. Actually, the obstruction appears to be more effective in the second hemisphere than in the first. This difference is obviously caused by the fact that, when the cortical object moves into the second striate area, parts of this area have been subjected to electrotonic action for a longer period than corresponding parts of the other area during the first half of the exposure.

It is a particularly interesting question how the current of a cortical object which moves, say, in the left visual cortex causes obstruction in the right visual cortex also. How does this current flow? From the point of view of physics, the most plausible assumption is that, in each location of the cortical object, the current passes directly across the longitudinal fissure into corresponding parts of the striate area on the other side. The distance is very short, and, although there are particular obstacles such as the dura, there is no reason to believe that these cannot be overcome. After all, our records show that currents from the same sources emerge on the surface of the head after overcoming far greater obstacles.

This reasoning applies without qualifications only to those parts of the two striate areas which face each other across the longitudinal fissure. The situation is different when those parts of the striate areas are considered which lie directly under the skull, and contain the cortical representation of the fovea. For geometrical reasons, the current issuing from more peripheral parts of one striate area can probably not affect the foveal part of the other striate area quite so much as it affects equally peripheral parts located on the other side of the median fissure, and also the foveal cortex on the side of the source. It is not entirely impossible that this explains an observation to which we have referred when discussing Records 6 to 12. With the active electrode placed in the median plane, the strongest deflection sometimes occurs when the cortical object has just entered the second hemisphere. This slight delay of the greatest deflection and the weaker response in the second half of the exposure as a whole, are, of course, entirely compatible facts. The tentative explanation which has just been offered actually derives both from the same functional principle as applied to the geometry of the visual cortex. However, not too much importance should be attributed to this particular argument; because, in many instances, the point of the greatest deflection does coincide with the fixation mark.[10]



RECORDS 20 and 21. Subject W. Responses to a moving object when the occipital electrode is placed above the left foveal cortex. The object moves from the left to the right.

[10] If it were true that the current of an object in one striate area has electrotonic effects also in the other striate area, presumably first of all in parts which correspond to more or less symmetrically placed regions of

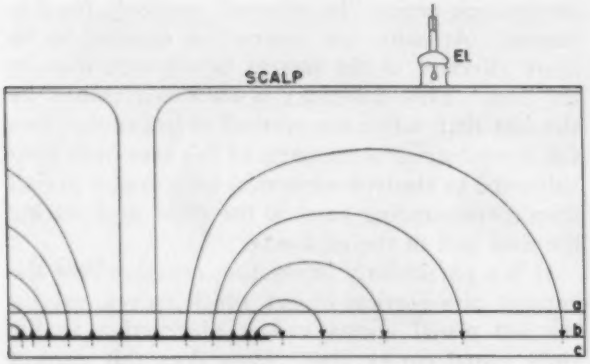


DIAGRAM I. The distribution of lines of flow within and in the neighborhood of a cortical object. *a,b,c* parts of the visual cortex, *b* representing the level of layer IV. The place of the cortical object is indicated on the left side by a stronger line. El. is the active electrode.

Since the experiments discussed in this section refer to localization within the visual cortex, it seems advisable at this point to consider in a more general fashion in what sense, and to what extent, our technique can be used for such purposes. It has been suggested to us that we attach large numbers of electrodes to the surface of the human head, and then try to register the currents which emerge everywhere from the cortex. So far as visual currents in our sense are concerned, this suggestion seems to involve a certain misunderstanding of the physical situation with which we are dealing in our work. The most important point is that, with the active electrode placed on parts of the scalp under which visual tissue is located, and with the second electrode attached at a considerable distance from these parts, records do not generally indicate what happens in the cortical tissue located directly under the active electrode. A simple diagram will show why they cannot do so. If we assume that the electromotive force which drives the current of an object through the tissue operates at right angles to the visual cortex, and that this force is located in the cortical representation of the object, then the lines of flow which pass through, and around, this object must be distributed as shown in the diagram (I [11]). The variation of the density of the flow is, as usual, indicated by a variation of the distance between a given line and its neighbors. For obvious reasons, the density is higher near the boundary of the object than it is in its middle, and in the more remote parts of the environment. Lines of flow which pass through the middle of the object in one direction are at the same time those which pass through the most distant regions of the surrounding tissue in the opposite direction. The diagram makes it clear that these lines of flow envelop all others. As a result, any differences of potential which are established on the surface of the head are necessarily differences along these particular lines of flow, the only ones which can reach the surface of the scalp. The characteristics of records which we take from the intact heads of human *S*s are therefore directly related only to this part of the current.[12] It follows that no conspicuous change may appear in our records when the state of cortical excitation under the active electrode is suddenly and radically altered by a movement of the cortical object. For the potential at this electrode is not directly determined by the local level of cortical excitation. Rather, this potential depends upon the distance between the electrode and the middle of the bright object as measured along the enveloping lines of flow—wherever this object may be located at the moment.

In the diagram, the cortical object is shown in a position in which it is not located under the active electrode. At the time, parts of the dark background are cortically represented under this electrode. Nevertheless, the potential at the electrode itself, on the surface of the head, is already determined by the lines of flow which emerge from the middle of the bright object, and then envelop all others. If now the cortical object moves to the right (in the diagram), there will be a moment at which, under the electrode, the cortical representation of the dark background is replaced by that of the bright object. Does any

the visual field, then it would follow that the size of many figural after-effects as observed by Köhler and Wallach (14) may be somewhat reduced by this factor. The authors often showed their two test objects in symmetrical positions on the left and on the right side. When the inspection object had been presented on one side, it was assumed that the test object on the other side was not appreciably affected. The present argument seems to show that this assumption may not be entirely justified. The second test object may also be affected; and, as a consequence, after-effects may actually be stronger than is shown in such tests.

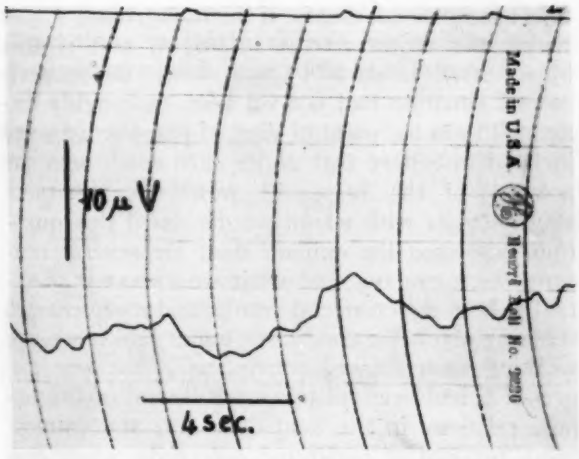
[11] In this diagram all anatomical detail has been omitted, and anatomical proportions have been changed so as to show more clearly how the lines of flow are distributed. No attempt has been made to represent this distribution correctly in a metrical sense.

[12] For the moment we ignore, of course, the possibility that currents also arise in the scalp, and that other active parts of the brain influence the form of the registered responses.

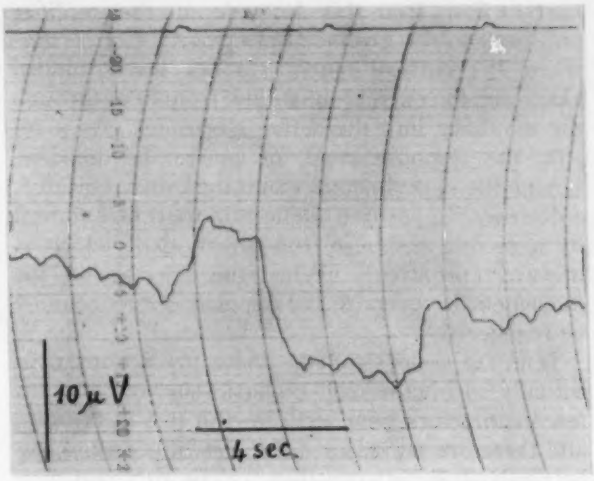
abrupt change as to the potential at the active electrode occur at this moment? Obviously not, because in this respect nothing happens but that the distance from the middle of the object to the electrode, measured along the enveloping lines of flow, is somewhat shortened, and that, therefore, the potential at the electrode is correspondingly raised. This is, of course, a perfectly gradual change. The same kind of change also occurred when the object had not yet arrived under the electrode, but merely approached this particular location. It follows that a sudden transition from one level of excitation to another and, as the diagram shows, an equally sudden reversal in the direction of the flow can take place within the cortex itself, while, at the same time, the flow which enters the electrodes is changed in a completely continuous fashion. For our purposes it suffices to know that the size of the response must vary when the distance between the cortical object and the active electrode is changed, and that in this way our records inform us about the location of the object.

If an experimenter were interested in the precise distribution of the enveloping lines of flow on the surface of the head, he should, of course, place many electrodes on the scalp. He could then discover what electrodes are placed on equipotential surfaces, and what others are not. But the potential at a given electrode would not generally tell him what happens in the cortical tissue which lies underneath.

Our argument is supported by the records which we have so far shown. For the most part, the



RECORD 22. Subject R. Diphasic response to a moving object. Both electrodes are placed above the foveal cortex, the active electrode on the left side. The object moves from right to left.



RECORD 23. Subject M. Diphasic response to a moving object. Both electrodes are placed above the foveal cortex, the active electrode on the left side. The object moves from left to right.

potential at the active electrode begins to rise as soon as the bright object appears in the periphery of the field. At this time, the cortex under the electrode is, obviously, in the state which corresponds to the darkness of the general background; and it remains in this state for several seconds, while nevertheless the potential at the electrode steadily grows as the object approaches the fovea.

In this discussion it has been assumed that the grounded electrode is placed at a considerable distance from the visual cortex. The experimental situation greatly changes if this distance is reduced, until eventually the grounded electrode is given a position in which it can also be affected by the moving object. If, for instance, the active electrode is attached to the crucial region on one side of the median plane, and the grounded electrode to the same region in a symmetrical position on the other side, the cortical object must pass first one electrode and then the other. Suppose the active electrode is placed on the left side, and the grounded electrode on the right side. If under these conditions the physical object moves across the fixation mark from the right to the left, its cortical counterpart must move first in the left, and then in the right, striate area. Consequently, the potential will first be raised under the active electrode, but a short time afterwards the potential at this place will be lower than that at the grounded electrode. Now, when the potential is higher under the active electrode, the

current flows from this electrode into the amplifier and back to the grounded electrode. Conversely, when the cortical object reaches the grounded electrode, the current must flow from here through the amplifier into the active electrode. As a result, the response must, of course, be diphasic. Under the experimental conditions just indicated, a downward (positive) deflection must be followed by a second response, the direction of which is upward (negative). When the direction of the movement is reversed, the opposite sequence must be registered.

It is far easier to make these predictions than actually to obtain such diphasic curves. Only a few centimeters now separate the two electrodes, and therefore no major difference of potential can be established between them. The situation may to some degree be improved by giving the electrodes a smaller diameter. When Records 22 and 23 were taken, the diameter of the electrodes was reduced to about 5 mm. The diphasic character of the responses is obvious in these records, although deflections correspond to only about 3 μV in Record 22 and to no more than 6 μV in Record 23. Obviously, the base line must be particularly quiet, if under such circumstances reliable results are to be found. In both instances, the active electrode was placed on the left side. The object moved from the right to the left during the exposure for Record 22, and from the left to the right when Record 23 was taken. The reversal of the sequence of the two polarities from Record 22 to Record 23 is perfectly clear, and so is the fact that in both records this sequence is the one which had to be predicted. In both curves, the transition from one polarity to the other occurs at about the time at which the object passes from one half of the visual field into the other.[18]

We have taken numerous records of this type. Very often the diphasic character of the responses is just discernible; occasionally, random fluctuations of the curve obscure this character so much that records cannot be used. Two other curves which exhibit diphasic responses have been reproduced elsewhere (13, Records X, XI).

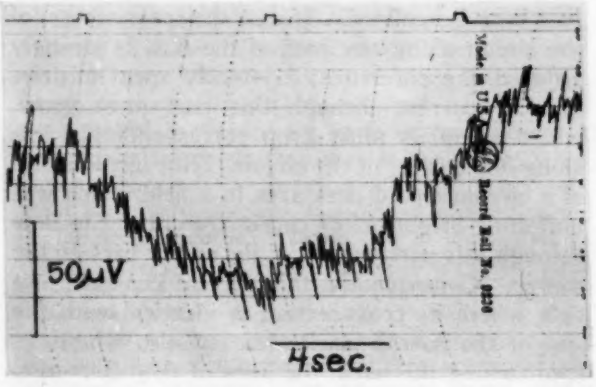
[18] We should like to mention that when Records 22 and 23 were taken the moving object was shown in an unusual fashion. A dark screen was placed before the *S* in a moderate illumination, and the moving object was the image of a bright vertical rectangle thrown upon this screen by a powerful projector. Under these conditions, the object did not look luminous; rather, its surface was perfectly white. We do not know whether the particularly satisfactory results of experiments 22 and 23 have been caused by this fact.

There is another question of localization which we have sometimes tried to answer in our experiments. Corresponding points of the two retinae are projected more or less upon the same points or small areas of the cortex. Actually, some observations in vision cannot readily be understood so long as spatial identity of the projections is supposed to be complete. Bárány and Kleist have discussed the possibility that nerve impulses from corresponding points in one eye and the other arrive at different levels of layer IV, although in about the same places as to the two other dimensions of the visual cortex (*cf.* 8, p. 524). If this were true, two electromotive forces would presumably be established when the nerve impulses from both eyes arrive. As a consequence, the question would arise whether the two forces have the same or, perhaps, opposite orientations in the tissue. For such reasons, we have occasionally compared records taken under conditions of monocular vision, right and left, and have also compared each with results obtained when the *S* used both eyes. So far such tests have always shown that the polarity of the registered currents is the same across the whole field, whether one eye or the other or both see the moving object. Now, if there were two separate forces, and if they had the same direction, one would expect records which are taken binocularly to give stronger responses by addition than do records taken monocularly—always supposing that conditions are otherwise the same. Our experiments have given no evidence to support this conclusion. But, then, it must be realized that it is extremely difficult to keep all other experimental conditions sufficiently constant while such quantitative tests are done. We must therefore leave this question unanswered.

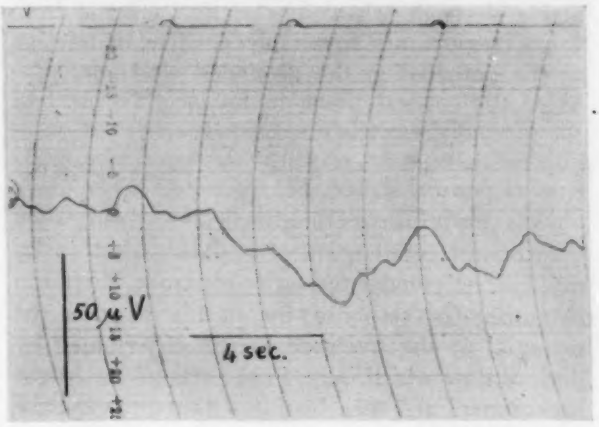
IV. THE CURRENTS OF DARK OBJECTS

What will happen if brightness relations in the field are reversed, that is, if the background across which the object moves is bright, and if this object itself is dark? Once, when the experimental situation had not yet been thoroughly examined from the point of view of physics, we were inclined to believe that under such conditions the polarity of the responses would be reversed. Physiologists with whom we discussed this question expressed the opinion that, for several reasons, we might not find what we expected. Actually, both experimental results and more careful thinking about the conditions under which we are working soon showed convincingly that our expectation had been mistaken. Reversal of brightness relations in the field does not, and cannot, cause reversal of registered potentials, when one electrode is attached to the region of the cortical fovea, and the other to the vertex. We have taken a great many records with the background fairly

bright and the object dark. Difficulties in obtaining clear responses are not at all greater under these circumstances than they are when a bright object moves across a dark field. We found that, whenever responses were otherwise satisfactory, their polarity was the same as that registered in the experiments which we have so far described. There can be no question: when a dark object passes under the active electrode, a potential is here established which is positive in relation to that at the place of the grounded electrode. Moreover, a weaker potential of the same sign arises long before the dark object arrives at the fixation mark; in fact, just as is the case with a bright object, the current generally begins to flow very soon after the dark object has appeared in the periphery. Record 24, taken with the active electrode in an occipital and median position, and with the grounded electrode placed on the vertex, gives a good example. When examining this response, nobody could guess from it alone that it is not a bright object on a dark ground which here has caused the response. Actually, the object was a vertical rectangle cut from black cardboard, the large screen behind it was white, and the illumination of the room was fairly strong. Agreement with responses obtained with a bright object seen against a dark background goes so far that, at the end of the exposure, there appears an after-potential of the same polarity as the original response, which, as in many of our curves, is immediately followed by a second. It will not be necessary to add a further record of precisely the same kind. We prefer to show that, since responses to dark objects are not in any essential respect different from those to bright objects, dependence of the responses on facts of cortical localization can also



RECORD 24. Subject E. Current of a dark object. The active electrode is placed in the median plane.



RECORD 25. Subject R. Current of a dark object. The active electrode is placed above the left foveal cortex. The object moves from left to right.

be demonstrated when the moving object is dark. When Record 25 was taken, the active electrode was placed on the left side of the median plane, and the dark object, the projected image of a thick wire which moved before a bright ground, went across the field from the left to the right side. These are the conditions under which positive results of localization experiments can be trusted, because a stronger current must be registered during the second half of the exposure. In Record 25 the (positive) deflection is clearly stronger during the second half of the response. Incidentally, in this curve the original response is also followed by an after-potential.[14] A further record taken when a dark object was presented will be shown in Section VI.

We will now explain why responses to dark objects on a bright ground must have the same polarity as responses to a bright object on a dark ground. The main point is that visual currents always issue from regions of the visual cortex which correspond to bright parts of the field, and that, in this respect, it does not matter whether such bright parts appear as circumscribed objects or as the background of dark objects.[15] But the potentials established at the

[14] In Record 25 the first mark on top is by a mistake slightly displaced toward the right. The exposure began more than .5 sec. earlier than the mark indicates.

[15] Strictly speaking, the situation is perhaps slightly more complicated; because, apart from electromotive forces which operate at right angles to the visual cortex in bright parts of the field, there may also be diffusion potentials of the kind first investigated by Nernst, which operate in a transverse direction (*cf.* 14, p. 324). But all authors who have studied bioelectric phenomena seem

active electrode when lines of flow emerge from bright regions are invariably positive in relation to the potential at the grounded electrode, provided the cortical tissue in the neighborhood of the grounded electrode remains inactive. Consequently there is no reason whatsoever why negative responses should be registered when dark objects move across bright fields. Even when the cortical counterpart of a dark object is located directly under the active electrode, there can be no negative response; for, in this situation, the potential at the electrode is still determined by lines of flow which issue from parts of the bright background, and then turn downward toward the dark object. Here again, it must be remembered that the currents which we register do not inform us about the state of cortical excitation under the active electrode. As has been explained in the preceding section, the polarity of these currents tells us only whether the potentials established at this electrode by electromotive forces in the bright parts of the field are higher or lower than the potential at the vertex electrode. With the vertex electrode placed at a sufficient distance, the former potentials are always higher.

In case this argument should not appear to be convincing, the same result can also be derived as follows. When a bright object appears in the periphery of an otherwise dark field, it surely establishes at the active electrode a potential which is positive in relation to that at the grounded electrode. Now suppose that there is a second bright object which appears simultaneously with the first, but on the opposite side of the field. Obviously, the second object also establishes a positive potential at the active electrode, and therefore the registered response must have the same polarity as is recorded when only one bright object is shown. But in this respect there can be no change if the two bright objects expand in the direction of the fovea, until eventually the *S* sees a bright field, in the middle of which there is a circumscribed dark object. Since during the course of these variations in the visual pattern it is always the flow from bright parts of the field which determines the potential at the active electrode, the polarity of the response cannot possibly be reversed when, as a final result, a part of the original black background is transformed into a dark object on a bright ground.[16]

To be precise, so far we have explained only why the polarity of responses to the appearance of dark objects in a bright field cannot be negative. But before a dark object is shown in such experiments, the *S* sees a very large bright field, and there is every reason to believe that this field establishes a weak positive potential at the active electrode (*cf.* Introduction, p. 293). Now, Record 24 clearly shows that whatever the potential at the active electrode may be in the absence of the dark object, upon the appearance of this object the potential is almost immediately raised. We must therefore now explain why the introduction of a dark object in a bright field gives rise to a higher potential than is established by the bright field alone. Our explanation can be given this simple form. Other conditions being equal, differences of potential are distributed along a circuit according to the cross-section which the flow has in its various parts. For equal stretches along the flow, small differences of potential are needed to drive the current through parts which have a large cross-section, while the potential must be more strongly lowered along parts in which the flow is more compressed. This rule must be kept in mind when the potential established at the active electrode by a bright field alone is compared with the potential which here arises upon the appearance of the dark object. So long as the dark object is absent, the current which emerges from the cortical counterpart of the large bright field has a fairly wide cross-section as it passes through this source; it has perhaps a narrower cross-section as it takes, in all directions, a transverse course between the source and the surface of the head; and it has an extremely wide cross-section on its way through large parts of the brain back to the source. Potentials along the circuit are distributed according to the rule which has just been formulated. If now the cross-section of the circuit along any part of the flow is strongly reduced, the difference of potential spent in driving the current through this part must grow, while potentials must drop correspondingly less along other parts of the circuit. But introduction of a circumscribed dark area in a field which was uniformly bright before causes the current to flow through this narrow part of the cortex back to the source. Consequently, this current contracts not only where its cross-section is identical with the size of the narrow dark area but also where, in approaching this area, the lines of flow converge,

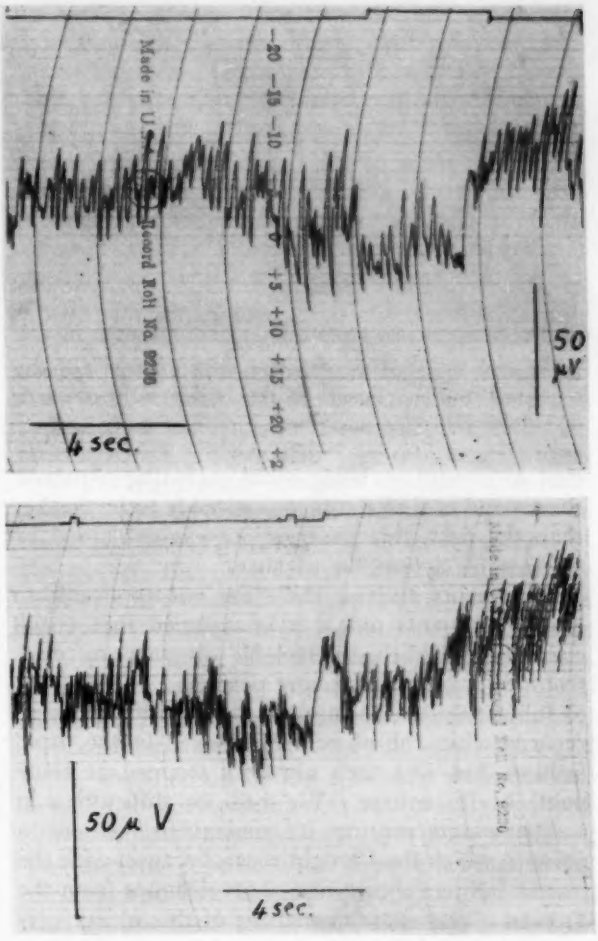
to agree that the latter forces are so weak that they may be ignored in comparison with the main sources of bioelectric currents.

[16] To repeat, in the present argument it is assumed that the grounded electrode is attached to the vertex, that is, at a considerable distance from the visual cortex. When both electrodes are given occipital positions so that diphasic responses are registered, a moving dark object may actually cause responses with reversed polarities.

and similarly where they emerge again, and then gradually diverge. The result is that a great difference of potential must be established between points at the beginning and at the end of this contracted part of the flow, while potentials along other parts of the circuit must now drop more slowly. The drop of potential in which we are especially interested is that from the bright parts of the field to the location of the active electrode, that is, along the stretch of the circuit in which the current flows before it begins to contract on its way to the narrow dark part. Obviously, along this stretch the potential now drops less than it did before the introduction of the circumscribed dark object. Hence, the potential at the active electrode must rise, and the current which flows through the leads must be increased, as soon as the dark object appears. We need not explain why this effect grows as the dark object approaches the fovea. The explanation follows directly from what has just been said.

Records 24 and 25 both begin with a short and weak negative deflection. One is tempted to relate this initial response to the fact that the object shown in these instances was dark. But it would not be safe to attempt an explanation of this detail in such terms. Very short negative deflections in the beginning of the response occur also when a bright object moves across a dark field. Since the negative response occurs in the beginning, it obviously refers to a strongly peripheral location of the cortical object. In this location, the current emerges from a place fairly deep in the median fissure. Now, when the distances from the source to the two electrodes are compared, they will be found to differ considerably less with the source deep in the median fissure than they do when the source has moved nearer the cortical fovea. But the drop of potential from the cortical object to the surface of the head, and to one electrode or the other, depends not only upon the distances as such but also upon the local impedances which must be overcome in the two directions. Therefore, if the impedance of the skull near the occipital pole should be greater than that in the region of the vertex (which may well be the case), the potential is perhaps for some moments lower at the occipital electrode than it is at the vertex, when the object appears far in the periphery. As a result, short negative deflections would tend to occur in the beginning of records. We do not, of course, maintain that this is the only way in which initial negative deflections can be explained.[17]

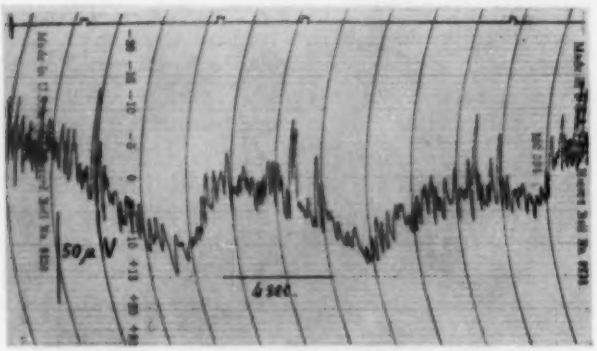
[17] In this report we generally refrain from discussing the short-lived phenomena which often accompany the beginning of exposures. Our equipment is not chosen for the purpose of studying such facts. We will merely remark that we sometimes find sequences of rapidly varying potentials in the beginning of our records, which resemble those first described by Bishop (3).



RECORDS 26 and 27. Subject R. Reduction of visual responses when the movement of the object is interrupted.

V. ELECTROTONIC EFFECTS

At several points in this report we have referred to the concept of electrotonus. Moving rather than stationary objects were used because it was assumed that the currents of the latter would establish a higher degree of this obstruction, and thus weaken themselves too fast for our purposes. For the same reason we avoided intense contrast between the brightness of the object and that of its background. Again, objects of some width were generally preferred to very narrow objects, because the dense flow which passes through narrow cortical areas must have particularly intense electrotonic effects. Also, when it was found



RECORD 28. Subject W. Reduction of a visual response when the movement of the object is temporarily interrupted.

that, all conditions being equal excepting sequence, the second half of the response tends to be weaker than the first, this observation was explained by electrotonic action "in advance."

Arguments such as these are not applicable to our experiments unless it is assumed that visual currents establish appreciable degrees of electrotonus within very short periods. We decided to subject this assumption to direct tests by taking records when the object first moved in the usual fashion, but was then abruptly stopped at some point of its course. We had no difficulties in suddenly interrupting its movement without a noise to which the *S* might react, because, with the physical object shown at a short distance from the *S*, even a very slow movement of this object corresponds to a sufficient velocity of its retinal image.

Record 26 gives a first example of the way in which a visual current reacts to the sudden transition from movement to rest. (When this record was taken, the active electrode was again placed in the region of the cortical fovea, and in the median plane; the grounded electrode was attached to the vertex. The same holds for Records 27 and 28.) The object was a bright projected bar which first appeared at an angle of 32° from the fixation mark, and moved with a retinal speed of about 5° per sec. to a position just before the fixation mark, where it was abruptly stopped. The deflection in the line on top gives the time during which the object was seen moving. Only a weak original response has developed during this period; nevertheless, the effect which interruption of the movement has upon the current is perfectly obvious, particularly because this reaction is almost instantaneous. Clearly, it is a small fraction of a second during which the response is virtually destroyed.

Record 27, taken from the same *S*, but on another day, shows the same reaction when the object is stopped a very short time after passing the fixation mark. The first mark on top corresponds to the first appearance of the object, the second mark to its coincidence with the fixation mark, and the beginning of the steady deflection to the moment at which the movement is suddenly interrupted. (In this experiment, the object was a vertical rectangle of white cardboard which moved before a dark screen in a moderately well illuminated room.) The reaction is quite similar to that shown in the preceding record. But does it really occur because the movement of the object is suddenly intrerrupted? Even when the object moves across the whole field, the response is often greatly reduced in the second half of the exposure, presumably by electrotonic action during the first half (*cf.* Section III). Actually, the reaction shown in this record cannot be explained in this fashion. The very common weakening of visual currents when the object has passed the fixation mark is never so abrupt as the collapse of the response in Record 27.

In our third example (Record 28), which was taken from another *S*, the original response is much stronger. In this test the (projected) object appeared at a distance of 36° from the fixation mark with a retinal speed of about 6° per sec., and was suddenly stopped about 7° short of the fixation mark. It was kept in this position for slightly more than 3 sec., and then moved across the remainder of its course. The weakening of the response upon interruption of the movement is striking, and so is the fact that the response remains weak while the object is stationary. After the movement is resumed, the deflection gradually returns to the level attained before the stationary part of the exposure. Obviously, however, the reaction caused by interruption of the movement is far less abrupt in this instance than it was in Records 26 and 27. The difference might be related to individual differences between the *S*s from whom these records were taken. We rather suspect that the speed of electrotonic action is not the same in all people; but we cannot be sure that in the present experiments this is the decisive factor. The speed with which the electrotonic obstruction is established is bound to depend also upon objective conditions. For instance, not only the intensity of the brightness difference between the object and its environment but also the width of the object must influence that speed, because these factors determine the density

of the current which causes the obstruction. Now, when Record 28 was obtained, the moving object was considerably wider than the objects used in the experiments of Records 26 and 27. This difference alone would fully explain why, in the present record, the transition from one level of the response to another occurs more slowly. The density of the current registered in Record 28 must have been fairly low.

In Records 26 and 27 the response seems to disappear entirely when the movement is interrupted. One might ask whether this fact is not disturbing for a theory which relates the perception of visual objects to the flow of corresponding cortical currents. Surely, the *S* still sees the object after its movement has been interrupted. Would it not follow that objects may be seen when there are no corresponding currents? This argument is based on a mistaken premise. What the present records demonstrate is by no means that when the object is stopped the cortical flow disappears; they prove only that the intensity of this flow is now reduced to a level at which the potential established at the active electrode no longer maintains a current which we can distinguish from random fluctuations. This may happen while at the same time the current along shorter lines of flow, those which do not enter the skull and the scalp at all, is only reduced to a somewhat lower intensity, but does not by any means disappear. Once more, responses registered from the surface of the head are directly related only to the weak flow which emerges from the middle of the cortical object, and has to overcome a particularly great resistance on its long way back to the source. If, under particular conditions, this flow becomes too weak to give a recognizable deflection, we can obviously not conclude that now no current flows through and around the cortical object.

The rapid decrease of the visual current when the cortical object is suddenly stopped has just been interpreted as an effect of greatly intensified electrotonic action. Most neurophysiologists will probably agree with this interpretation. So-called "retinal fatigue," of which one might also think in this connection, is not established so fast, especially when stimulation is as moderate as it was in the present experiments, most of all in that of Record 27. But if it is intensification of electrotonus which here weakens responses so strongly, then our suspicion that the investigation of visual currents is generally made difficult by this obstruction appears fully justified, and the measures by which we have tried to reduce its size will have to be used also in further studies of this kind. Even if electrodes were attached to the surface of the brain, responses might rapidly be weakened if no such measures were taken. More particularly, we believe, first experiments in this field ought always to be done with moving objects.

However, the obstruction is not likely to disappear altogether when moving objects are presented. To be sure, according to our present records, electrotonic effects are greatly enhanced as soon as the movement is interrupted. But these records show at the same time that the intensification is a very rapid process, and that therefore some electrotonic action must also take place when the current flows only for a very short time in given parts of the tissue. This means, of course, that when objects move with the fairly low speeds which we are using in our experiments, their currents must establish at least a certain amount of the obstruction. Moreover, however weak the local effect may be in each position of the object, the obstruction must soon grow by summation, because later points in its course have for some time been subjected to the same influence before the object actually reaches these points. Thus the concept of electrotonic action "in advance" is justified by the present records.

Electrotonic action of a current is not restricted to the short period during which the weak flow accessible to our technique is so strongly reduced. Physiological investigations have shown that, if the current continues to flow, electrotonus increases, and that it may then persist far beyond the duration of the flow. We hope that the latter fact can also be demonstrated with our method. But even our present records establish a close connection between the physiology of visual processes and so-called figural after-effects. The records prove that the currents of stationary visual objects raise strong obstacles to their own flow. It follows that the distribution of such currents must soon change: their density must be reduced in parts in which the flow was first particularly intense, and it must grow in parts in which the flow was first weaker. Numerous studies of electrotonic action have shown that redistributions of this kind actually occur. Now, the theory of figural after-effects which we have given elsewhere (14) rests only on a few physiological premises. It is assumed that cortical visual objects are sources of currents, that these currents establish electrotonic obstructions roughly in proportion to the local density of the flow, and that, as a consequence, their distribution changes as just indicated. Apparently, these premises are now well verified. For, once it has been shown

that cortical visual objects are actually sources of currents, and that these currents have strong electrotonic effects, the redistributions to which we have just referred need hardly be demonstrated in special experiments. Such redistributions must occur if we accept what electrophysiologists regard as well-established facts. But, to repeat, at one point the theory is still in need of direct physiological confirmation. Figural after-effects have proved to be extraordinarily persistent. Therefore, even if electrotonus in general is undoubtedly a very persistent condition, one might wish to see it clearly demonstrated that the obstructions caused by visual currents also survive for considerable periods when these currents themselves no longer flow. We are now preparing experiments of this kind.

With regard to a further point, the connection between Records 26 to 28 and psychological theory is even closer. Visual distortions which strongly resemble figural after-effects are often observed at once, that is, when no period of prolonged inspection of an object has preceded the observation (14, pp. 352 ff.). As an explanation, it has been suggested that a certain amount of electrotonic action may occur within a fraction of a second, and that, therefore, the explanation of such distortions may be found in corresponding redistributions of the currents in question. If this were true, the electrotonic effects of visual currents would contribute to the characteristics of visual percepts in general; because it is obviously impossible to refer to such immediate electrotonic effects only when some particular distortions have to be explained. The suggestion that appreciable degrees of electrotonus can be established almost at once was originally made with some reserve, precisely because it leads to this radical consequence. At present, no reason seems left for this reserve; for our records demonstrate immediate electrotonic action precisely in this sense. It follows, of course, that some aspects of normal vision must actually be influenced by electrotonic obstructions, and that such facts can be understood only if this is recognized.

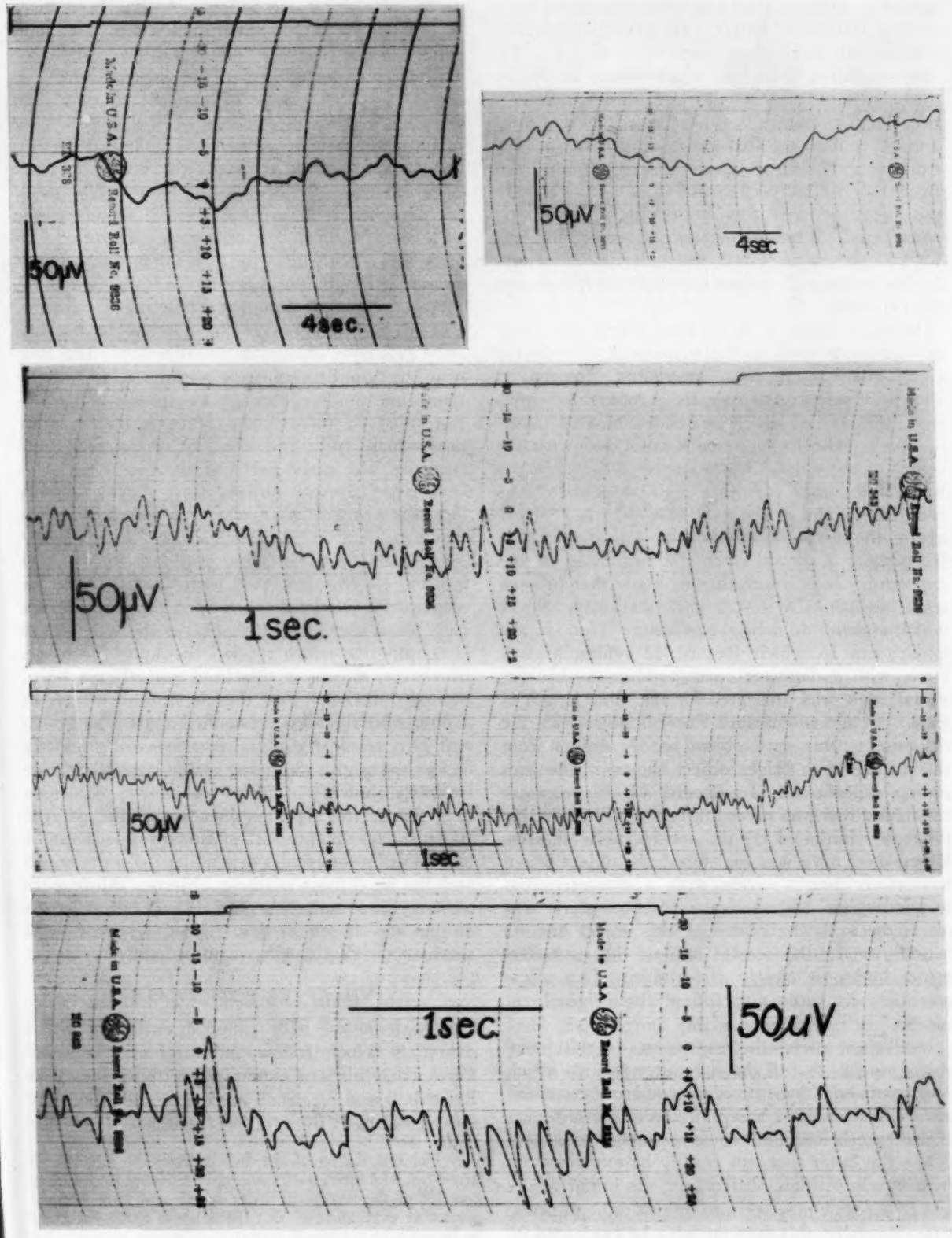
Since electrotonic effects of some degree must also be established when an object moves with moderate speed, one can safely say that vision in general is a "damped" process. On the other hand, our records prove that the damping is immediately enhanced when a cortical object comes to rest. It is therefore not an astonishing fact that moving visual objects have a greater stimulating power in man and many animals that have stationary objects. One is at first tempted to infer that movements are associated with particular processes which stationary situations do not arouse, and that it is such additional events which make movements especially effective causes of reactions. Now, there are the very best reasons for assuming that the processes underlying perceived motion actually contain components which are absent when the perceptual field is stationary. It may also be expected that sooner or later some phase of the currents which we register will prove to be specifically related to the fact that we show moving objects in our tests. Even so, it must be recognized that, quite apart from particular physiological events which may accompany the experience of movement, reactions to moving objects are likely to be stronger than those to stationary objects simply because there is less electrotonic damping in the former case.

VI. THE CURRENTS OF STATIONARY OBJECTS

Since the current of a visual object is sharply reduced when the movement of the object is suddenly interrupted, one cannot expect strong responses from objects which are stationary to begin with. We have nevertheless taken many records under these conditions. Difficulties were perhaps not quite so great as might have been anticipated, because now the various measures by which electrotonic action can be limited were taken with particular care.

It is essential that convincing records of cortical currents be also obtained when objects are stationary. For, as we have just mentioned, the currents registered when objects move across the field may contain components which are characteristic of visual motion as such. But if this is true, one might ask whether the registered currents could not be caused altogether by the fact that we work with moving objects. This suggestion becomes untenable if, as a matter of principle, currents can also be recorded when stationary objects are seen.

Records 29 to 33, taken from four different *S*s, show such responses. When Record 29 was taken, a straight bar, shown by opaque projection, was presented. The bar, the brightness of which was quite low, crossed the fixation mark. The response shown in this record is a weak current which has the same polarity as currents registered when objects move. There is a short delay in the beginning. After the exposure, after-potentials seem to follow. Record 30 gives the response to a vertical bar of somewhat higher intensity which appeared on one side of the fixation mark for no less than 18 sec. This response is



RECORDS 29–33. Subjects R., M., P., and B. Currents of stationary objects.

probably stronger than any other registered by us with a stationary object. Its greatest deflection corresponds to slightly more than 20 μV. The short negative deflection which occurs in the beginning is perhaps not a random fluctuation; it occasionally appears in other records of this kind, and is sometimes stronger than in the present example (*cf.* also p. 315, footnote). Quite apart from this initial peculiarity, it is clear that the positive response reaches its greatest size only after some time. When the object has been in the field for a considerable period, the deflection decreases. In this instance also, after-potentials seem to follow the exposure.

When Records 31 to 33 were taken, very large objects were used with the purpose of reducing electrotonic action to a minimum. Record 31 shows the response to a square of moderate brightness, the side of which subtended a visual angle of 155°. The fixation mark coincided with the center of the square. The length of the exposure was about 5 sec. (A very high paper speed was used in this and in the next two records.) Here again, the response reaches its full size only a fraction of a second after the beginning of the exposure. It is interesting to know that in such tests one can also use a dark stationary object which appears on a bright ground. Thus, in the experiment to which Record 32 refers, a dark circle with a diameter corresponding to 130° of visual angle was presented for the same length of time. In agreement with Records 24 and 25, the response to this dark object hardly differs from that to the large bright object shown in the preceding experiment. In Record 33, the response resembles the two preceding ones, although this time the initial delay is considerably shorter. When this curve was registered, the object was a moderately bright circle with a diameter corresponding to 36°, and the fixation mark was placed between the center of this object and its boundary. In the second half of the exposure, the α-rhythm is clearly discernible. Two after-potentials in succession follow the original response.

In all these curves the response to the stationary object reaches its full size only after periods which vary from about one-fifth of a second to several seconds. Also, without exception the response begins to decrease before the exposure comes to an end. While the latter fact can readily be explained by growing electrotonic obstruction, the initial weakness of the flow may seem to offer a difficult problem. Actually, this fact can also be explained in terms of electrotonic action. As has been mentioned before, in our experiments with stationary objects we always used either weak or very large objects in order to protect the current which issues from the middle of the object, and then reaches the active electrode, against this weakening effect. Apparently, we went too far in this respect, and thus registered currents, the initial intensity of which was extremely low. Now, while under such conditions the weak current from the middle of a cortical object has only faint electrotonic effects, this is not true of the more intense flow which passes through the object near its boundary. Here, electrotonic action must be much stronger. But in such a situation the current as a whole must gradually change its distribution. The flow near the boundary must be weakened, and at the same time the flow through the middle of the object must be intensified. Since it is the latter flow which determines the size of the registered response, the early parts of records taken with very large or weak objects must show gradually increasing deflections—which is precisely what they do.

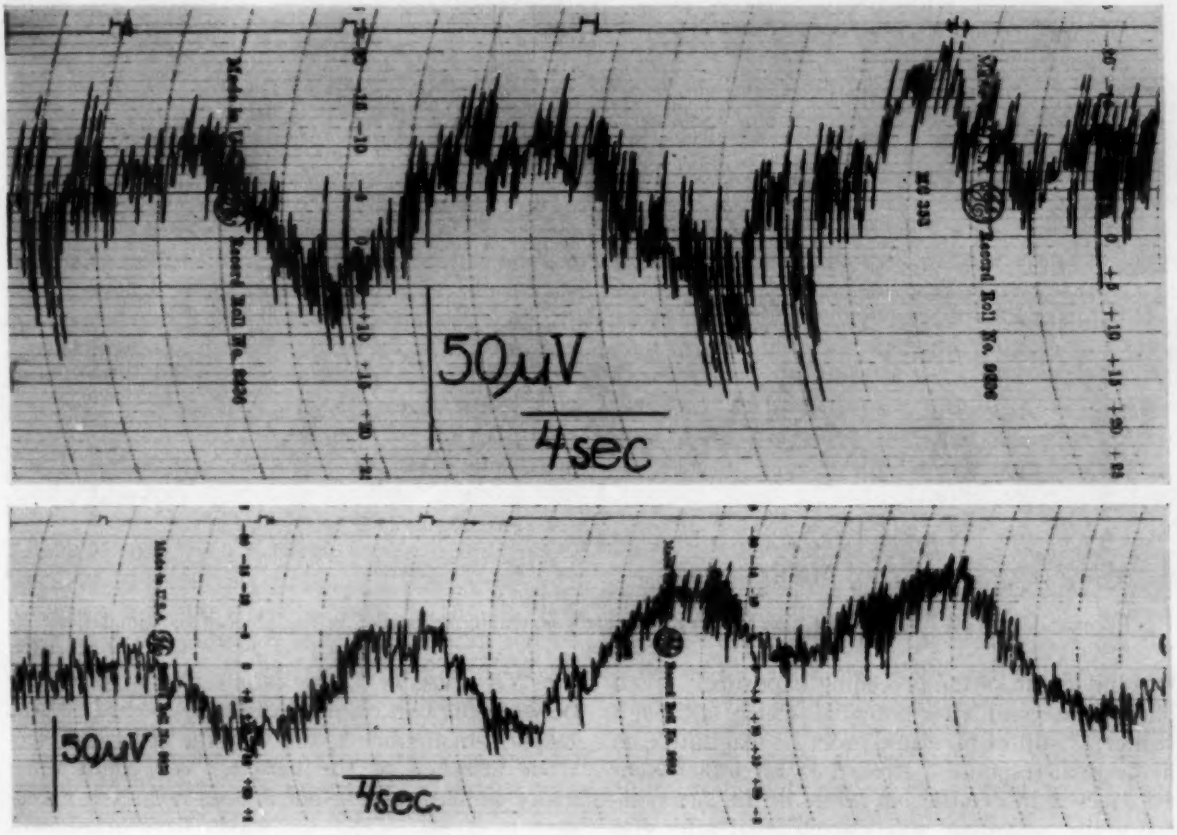
It seems to us an important fact that in this fashion electrotonus may occasionally intensify responses. Obviously, however, this can happen only when the flow which causes the obstruction is not the one which reaches the active electrode. Moreover, effects of this kind will always be of limited duration. For, the electrotonic effects of a flow which is thus intensified must also grow; and, as a result, the initial increase will gradually be lost again. In our records this later stage, too, is clearly shown.

If this is the right explanation of the present records, experiments with stationary objects ought to give different results depending upon the width and the brightness of these objects. More particularly, it is quite possible that, if optimal conditions are chosen in this respect, responses will reach their full size almost immediately.[18]

VII. AFTER-POTENTIALS

Repeatedly we have called attention to after-potentials which follow the original responses. Particularly obvious examples are those shown in Records 6 and 7. Such after-potentials have the same polarity as the original responses. It seems

[18] The retardation of the full response in Records 29 and 30 is, of course, partly an artifact caused by the slow reaction of the recording device which was used in these particular experiments. But the delay is much too great to be entirely explained in this fashion.

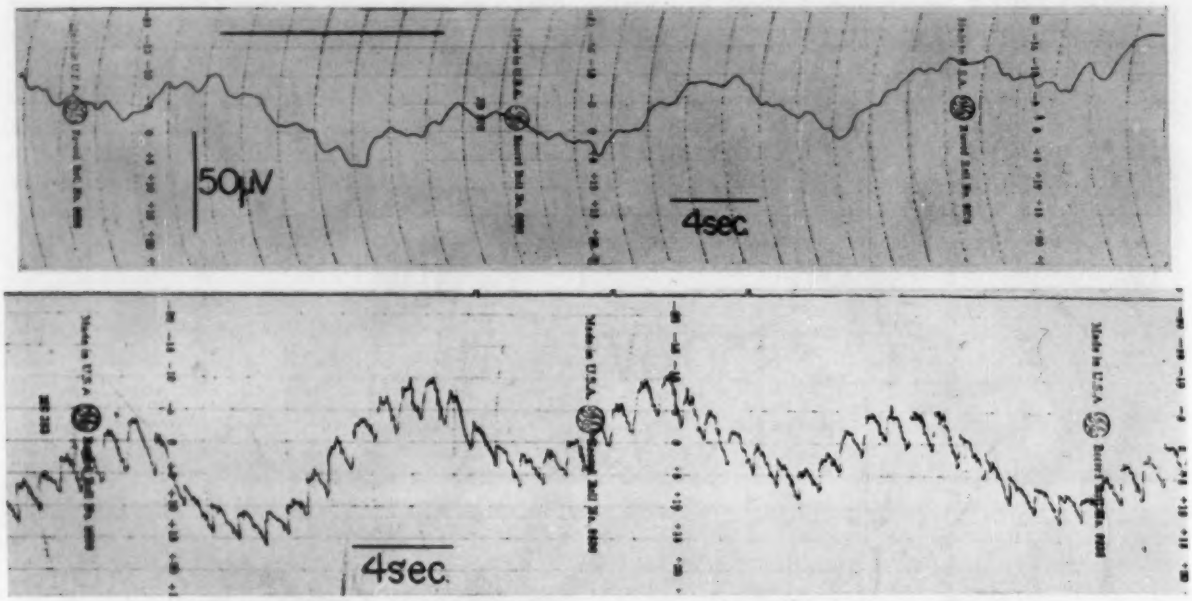


RECORDS 34 and 35. Subjects R. and E. Visual responses followed by strong after-potentials.

probable that, after an exposure, the tissue remains for some time in a changed state, and that the current of an after-potential accompanies processes which tend to establish the original state. But if this is true, after-potentials are likely to repeat themselves. For, as a matter of principle, the current of an after-potential must affect the tissue in the same way as the current of the original response has done before, and as a consequence a second after-potential may now follow the first. In our records, we have shown several instances of this kind. Actually, it will soon be seen that under certain circumstances much longer sequences of after-potentials may originate. If our explanation of the second after-potential is correct, such further repetitions are, of course, quite understandable.

Sometimes, the currents of after-potentials are very strong. Their intensity seems to grow when a *S* is given a great many exposures. Eventually, their size may approach that of the original responses, although this does not happen with all *S*s. Quite often, their duration is increased at the same time. As a result, records may now look as though the original responses were immediately repeated. Records 34 and 35 are striking examples of this phenomenon.[19] They were taken from different *S*s, both of whom had been given many exposures just before the tests to which these records refer, and also during preceding sessions. (The active electrode was placed in the median plane and in the region of the cortical fovea; the grounded electrode was attached to the vertex. The objects were bars of white cardboard which moved before a dark screen in a fairly well illuminated room.) In both records, the original response is strong and of the usual shape; the deflection is about symmetrical, and its greatest size is reached as the object passes the fixation mark. In Record 34 the first after-potential has the same size as the response itself, and its duration is not very much shorter

[19] In Record 35, the persistent deflection of the signal line on top, which begins after the exposure, is of accidental origin. Records 34 and 35 are covered with α-waves which have partially fused in these reproductions.



RECORDS 36 and 37. Subjects M. and E. Trains of after-potentials registered in the absence of stimulation.

than the length of the preceding exposure. Then follows a second after-potential which, although weaker, is still of the same order of magnitude as the original response. Record 35 resembles Record 34 even in details; but while in the latter instance the recording was discontinued after the second after-potential, Record 35 clearly shows a third after-potential of considerable size.

These curves may establish a connection between the ordinary after-potentials, of which we have given many examples, and far more conspicuous effects of often repeated exposures. When many experiments have been done, slow waves of considerable amplitude can sometimes be registered which follow one another with great regularity in the absence of exposures, and for very long periods. For instance, one minute after Record 35 had been taken, the *S* was asked to fixate the usual mark, and recording was resumed without any exposures. The result was a curve in which slow waves, each about as long as the after-potentials in Record 35, followed one another. Records 36 and 37 may serve as examples of this phenomenon. Even when both electrodes are attached to the region of the cortical fovea, as they were when Record 37 was taken, the amplitude of the waves may be large. In this record it varies between 15 and 30 μV.[20] (The very regular shorter fluctuations in this record represent the *S*'s pulse.) There is no major difference between the two records, although Record 36 was taken from another *S*, and with the grounded electrode attached to the vertex. We could show many similar curves; but it does not seem necessary to do so. Once the waves have originated, they can be registered with the greatest ease. Strangely enough, difficulties with which one is sometimes confronted when recording responses to actual exposures hardly arise when such waves are being registered.

In our investigation, this effect of too many tests has so far been treated as a disturbance rather than as a possibly important physiological fact. Obviously, once the waves have appeared in a *S*, it is impossible to obtain from him further reliable records of visual currents, because the waves resemble visual responses so strongly. It is fortunate that early records from new *S*s never exhibit the phenomenon. Moreover, with several individuals no such waves have ever developed. The fact that individual differences play an important part in this respect follows from several observations. One of our *S*s was given more exposures in more sessions than any other; nevertheless, no such waves ever appeared in his records. With only one *S* the waves developed during the first experimental session, to be precise, after 17 exposures; with a second *S*, they began to appear toward the end of the second session, after altogether 48 exposures. Without any ques-

[20] By mistake, no calibration is given in the part of this record, which we here show.

tion, not only the exposures of a given session are effective; rather, such exposures leave a disposition, which may then be strengthened when many further experiments are done in later sessions. The disposition survives rest periods of more than one day. On the other hand, we have never registered waves of this kind when work had been interrupted for several weeks, even if before such rest periods the waves had made the *S* entirely useless for our experiments.

Since we did not want to lose *S*s in this fashion, but would have lost them in any attempt to study the waves as such, several questions which here arise cannot yet be answered. We do not know, for example, how long, once the slow oscillations have appeared, they may continue without further exposures. Obviously, this question is not identical with the one which refers to the persistence of a mere disposition. It does not seem probable that the waves as such persist for very long periods, although they may readily be reestablished after a few days. We also do not know whether any particular influences would make the waves decrease or disappear. Again, no precise statements can yet be made about the number of individuals in whom the phenomenon easily originates. About six among our 26 *S*s may be counted in this class; but many among the remaining twenty have not been given so many exposures and sessions as would appear necessary for a test.

Only cautious statements are so far possible also with regard to a further point. Ordinary after-potentials have the character of fairly short waves. There seems to be no particular relation between their length and the duration of the preceding exposures. As has been mentioned before, however, the more persistent waves which may develop after many exposures are much longer. Now, in Record 36 the average length of the exposures given before this record was taken is indicated as an horizontal line above the curve. This length corresponds to slightly more than 10 sec. On the other hand, the maximal deflections downward of the waves in this record are so well defined that the distances between them can be measured. The length of these distances varies little, and amounts on the average to 11 sec. Thus, the length of the waves is about the same as the mean duration of the exposures. In Record 37 the duration of the preceding exposures is given on top by operating the automatic marking device. This duration is 10.6 sec. When the distances between the greatest deflections of the waves are measured and averaged, their length is found to be about 10.8 sec. Again, the two periods are approximately equal. Obviously, such agreements need not mean very much, so long as the length of the exposures is not kept strictly constant in a given session, and then widely varied from one session to another with sufficiently long rest periods between the sessions. Only if under such circumstances the length of the waves were to vary correspondingly, could it safely be said that this length tends to be the same as that of the preceding exposures because of a causal connection. In this respect, we can offer only one fact. With one *S*, the waves developed once when the length of the preceding exposures had, on the average, been 12 sec. The period of the spontaneous waves was then 11.7 sec. After a rest period of several weeks, the same *S* was given shorter exposures. Their length was now 8.3 sec. When after many trials waves appeared again, their average length was about 7 sec. We are not, of course, convinced by this one observation under changed conditions. It still seems possible that, while the length of the waves grows when the state of the tissue is progressively altered by more and more exposures, this length agrees in our experiments with the length of the preceding exposures only by accident. Obviously, greater variations of exposure times and repetition of similar experiments with other *S*s are now indicated.

In the future, it will also be advisable to vary the position of the electrodes when the slow waves have appeared. It may seem plausible enough to assume that the waves originate in the visual cortex. Strictly speaking, however, present evidence does not prove that they do.

Quite apart from such problems, the occurrence of fairly strong currents in the absence of exposures raises an important question. These currents do not seem to be accompanied by phenomenal data. It may follow that cortical currents have phenomenal counterparts only if their relation to other physiological facts fulfils particular conditions.

VIII. AUDITORY CURRENTS

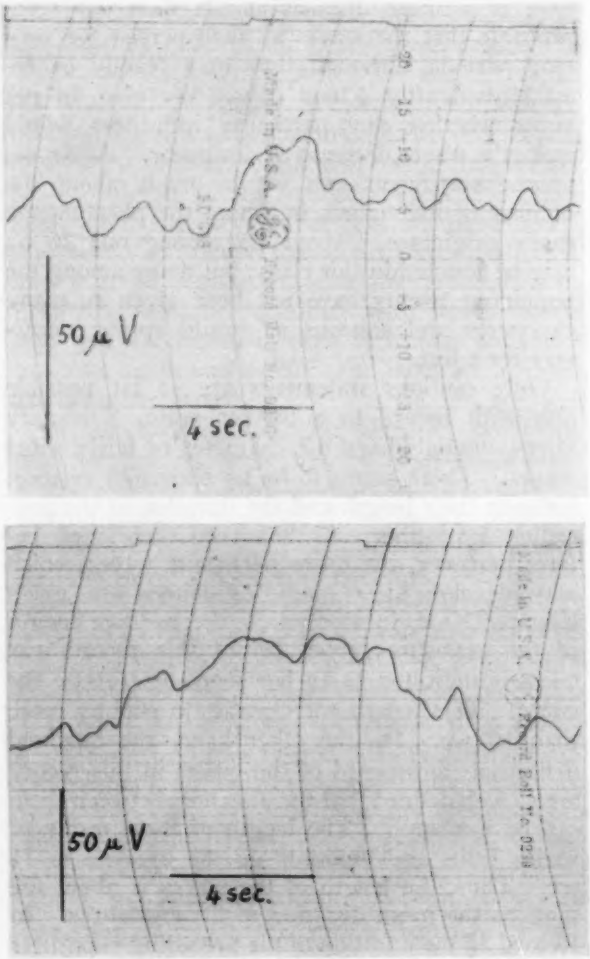
We did not restrict our work to the recording of visual currents. Attempts were also made to register currents under conditions of tactual stimulation, during voluntary movements, and when *S*s heard noises or tones. In the former cases, the active electrode was attached to regions under which parts of the sensorimotor cortex are located;

in experiments on hearing, it was placed in positions which probably corresponded to the auditory cortex. It is not, of course, an easy task to find the right locations in human *S*s when only the surface of their heads can be seen. Nevertheless, smooth potentials have repeatedly been recorded in all these instances. More specific statements about currents related to voluntary movements and to tactual impressions must be postponed until the conditions under which such currents are registered have been more widely varied. In hearing, on the other hand, some facts now appear as fairly well established. Good responses are often obtained when the active electrode is placed a few centimeters above the meatus, although the optimal location in such terms may not be precisely the same for all *S*s. The grounded electrode we always attached to the vertex. Under these conditions, the responses to auditory stimulation have the following form. After a short and spike-like positive excursion, which is not invariably present, a negative deflection follows. Sometimes this negative potential disappears after a few seconds of stimulation; but in many instances it persists for longer periods, and may continue slightly beyond the end of the sound.

When the negative response disappears during stimulation, it often does so quite suddenly. In some records, the negative deflection is immediately followed by a positive swing, which may then persist for some time. We are not convinced that such changes are always caused by factors intrinsic to the auditory situation as such. The peculiar orientation of the human auditory cortex in relation to the surface of the head is almost bound to complicate results.

When Record 38 was taken, a tuning fork (256 cycles) was struck at the moment where the line on top first deviates upward. The curve shows a negative response which develops very fast. Actually, the current grows even faster; for, when this record was taken, we used our slow galvanometer.[21] In this curve, the rapid decline of the response after about two seconds may be caused by the fact that the intensity of the tone itself rapidly decreased. Record 39 shows the reaction to the moderately intense sound of an old automobile horn. Again, the negative deflection begins almost immediately. But in this instance it does not entirely subside until some time after the end of stimulation. It may, of course, also be an after-potential which makes it appear as though this response continued beyond the end of stimulation. For, sometimes after-potentials follow the main responses in hearing just as they do in vision. When they do, their polarity is again the same as that of the original currents, which means, in the case of hearing, negative in relation to the grounded electrode.

Records 38 and 39 are taken from a series of preliminary tests with several *S*s, in all of which the potential at the active electrode was found to be negative in relation to the vertex. More recently, experiments have been done under conditions of better controlled stimulation. An earphone connected with an oscillator served as the source of sound. For the most part, this tele-

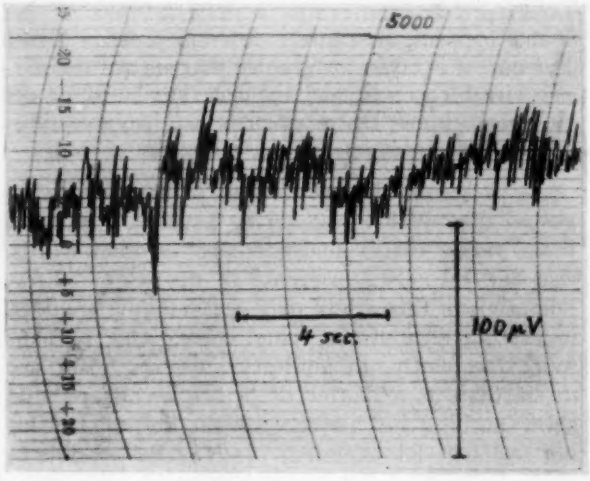


RECORD 38 and 39. Subjects M. and S. Auditory currents.

[21] For the same reason the short positive deflection which occurs in the beginning of many auditory records can also not appear in Records 38 and 39.

phone was not in contact with the head; rather, it was suspended near the contralateral ear. When Record 40 was taken, the active electrode was placed on the left side of the head, in the position which we have indicated. Our fast galvanometer was used. The earphone produced a strong tone of 5,000 cycles which was continued for about 5 sec. The curve, which is covered with α-waves, shows a slight drift upward; but the initial spike-like excursion downward and the sharp negative response by which it is followed are clearly recognizable. This negative deflection remains constant almost during the whole period of stimulation. Its size corresponds to about 20 μV.

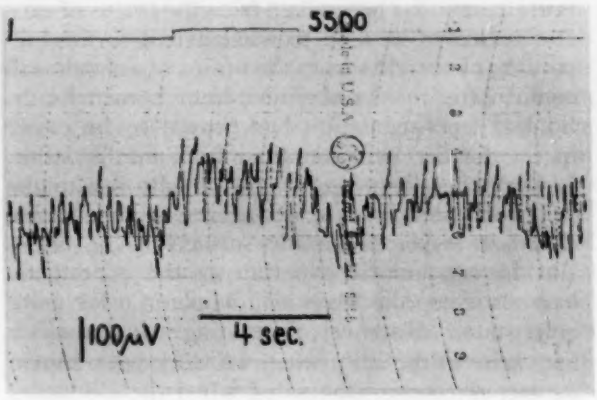
Record 40 must now be compared with Record 41, which was taken when a tone from the same



RECORD 40. Subject Ec. Auditory response to the frequency 5,000∼.

part of the scale (5,500 cycles) was given for 4 sec. While Record 41 greatly resembles Record 40 as a curve, its origin is actually quite different. We have chosen it from a series of similar records recently obtained by Dr. W. D. Neff and the senior author. The active electrode was attached to the left cortex of a cat, in this particular experiment to the posterior ectosylvian gyrus, from which the dura was removed.[22] The grounded electrode was attached to exposed bone of the skull. Technical details such as the adaptation of the electrodes to the new situation need not now

[22] Records were taken from various positions within, and in the immediate neighborhood of, the auditory cortex. We do not claim that this particular location has any special relation to the frequency 5,500 cycles, or that it is part of the primary auditory cortex.



RECORD 41. Auditory response registered from the exposed brain of a cat. Frequency 5,500∼.

be discussed. The experiments are being continued, and measures of this kind will be described elsewhere. For amplification and recording the same instruments were used as in our experiments with human *S*s. As an anaesthetic, Dial was given in a dose slightly smaller than is customary, because responses seemed to decrease with larger doses. In Record 41 the response begins with the same spike-like deflection downward as is often found in auditory responses recorded from human heads. This short reaction is immediately followed by a current of negative polarity which hardly varies during the remaining period of stimulation. Actually, it persists slightly beyond this period. While the over-all appearance of this response from a cat's cortex is very similar to that of the response shown in Record 40, there are also important differences between the two curves. In the first place, the spontaneous rhythm registered under Dial from the cat's brain has a lower frequency than the α-waves of Record 40. Secondly, the response in Record 41 is far stronger than that in Record 40. When auditory currents were registered from the cat's cortex, a much lower gain was used (*cf.* the calibrations). Thus in the record from a human head the main deflection corresponds to a voltage of only 20 μV, while in the response from the brain of a cat the voltage amounts to 100 μV.

In all tests which have so far been done with cats, the main auditory response has been negative. When the duration of stimulation is increased, this response may gradually weaken; but on the whole, there is less evidence of electrotonic obstruction in such responses to sounds than we find in the visual responses of human *S*s. No records of visual cur-

rents have so far been taken from the brains of cats. It remains to be seen to what extent further recording of currents from the brains of animals will confirm the results obtained from human heads. So far experimentation has proved to be easier in the former situation than it is in the latter. In part, the advantage is undoubtedly due to the fact that potentials are stronger when the active electrode is placed on the surface of the brain. But in our animal experiments the anaesthetic may also keep the brain as a whole in a far more quiet state. Moreover, fluctuating voltages which may arise in the scalp when we work with human *S*s are, of course, eliminated when the electrodes are attached to the brain or to the skull.

IX. DISCUSSION

While the experiments described in this report do not tell us how the registered currents originate, they tend to eliminate some explanations which have been suggested. In the following, we will mainly consider the currents which accompany visual perception, although we must sometimes refer to the fact that characteristic responses to tones have also been recorded.

According to some explanations, the registered currents are not directly related to visual perception at all. A first interpretation of this kind is that the currents accompany eye movements of the *S* rather than the presentation of visual objects as such. We cannot accept this interpretation. While the eyes tend to follow moving objects, this tendency can easily be suppressed when a single object is shown in slow motion. Under these circumstances, any sensible person who is asked to do so will fixate the prescribed mark fairly steadily. Moreover, it will be remembered that we have taken records of the currents which flow when the eyes are actually turned to the left or to the right, upward or downward. With the electrodes placed on posterior parts of the head, such currents are related to the position of the eyes in the head rather than to the movement by which a certain position is reached. Smaller deviations from the resting position are accompanied by weaker currents. Now, when an object moves across the field, the intensity of the registered current is maximal more or less at the time at which the object passes the fixation mark. If, at this time, the eyes were to follow the object, the size of their deviation would, of course, be very small. Consequently, accompanying currents would be particularly weak in this region. It is therefore not a plausible thesis that the currents shown in our records can be explained in terms of eye movements.

We should like to add that when the two electrodes are placed one behind the other, as they have been in most of our experiments, there are hardly any responses to eye movements so long as these movements are directed to the right or to the left. But since in the tests which we have here described the objects moved in an horizontal direction, involuntary movements of the eyes would generally occur in this direction, and would therefore have no appreciable effect on the records.

It is, of course, possible that, occasionally, the retinal image and the cortical object are actually displaced by an eye movement. Such a displacement would probably affect the registered current. But if this happens, an entirely different problem arises. For now it is taken for granted that we are registering visual currents, and the role attributed to eye movements is only that of events which may sometimes disturb the recording of such responses.

As a second interpretation, it has been suggested that we are registering psychogalvanic reflexes rather than visual currents; in other words, that the responses originate in the scalp. Now, potentials of this origin may occasionally be added to the currents of the visual cortex and, perhaps more frequently, to auditory currents. But the thesis that, as a matter of principle, our records show nothing but psychogalvanic responses cannot seriously be defended. Is it really possible to obtain five separate psychogalvanic reactions within half a minute when within this period a simple line crosses the field five times in succession? And yet, under such conditions we have repeatedly registered five separate responses. Quite apart from this point, why should it be necessary to keep the active electrode within the narrow area of the scalp, under which visual tissue is located? According to the present suggestion, the registered currents do not issue from the brain. It should therefore hardly matter where the electrode is placed in relation to particular parts of the brain. Again, why should our records reveal that the intensity of the registered currents depends upon the position of the active electrode in relation to more specific regions of the visual cortex? This interpretation also overlooks the fact that the latency period of psychogalvanic responses to visual stimuli is fairly long. The

currents which we register often begin to flow when the object has not been in the field for more than a fraction of a second. But the latency period of psychogalvanic responses to visual stimulation is larger than a full second; in fact, according to some measurements, it amounts to two seconds.

Dr. Gerard's question (7, pp. 236 f.) whether our records could not be explained in terms of electric reactions which accompany vascular responses of the brain is, of course, entirely justified. But, in the absence of specific evidence in this respect, we hesitate to believe that the registered currents actually originate in this fashion. Again, the temporal characteristics of the registered flow may cause difficulties. Does the electric phase of vascular responses develop as fast as the currents which we register? Moreover, would vascular responses in a given area follow as many as five exposures within half a minute? If we are not mistaken, a problem also arises from the fact that visual currents and auditory responses have opposite polarities.

According to a further explanation, the registered currents are actually related to visual processes, although not to processes in the visual cortex. This would be the case if the currents issued from the stimulated retinae. When discussing our records with scientists who have ample experience in the study of retinal currents, we have sometimes raised this question. Their verdict has been that currents of the stimulated retinae could not be recorded when the electrodes are given the positions in which they are placed in our experiments. Also, if the currents issued from the retinae, why should our findings agree with particular facts of localization within the visual cortex? More generally, why should it make such a great difference whether the active electrode is attached over the region of the cortical fovea, or, say, one centimeter outside this region? If appreciable potentials of retinal currents were established in the occipital scalp, the variation of such potentials when the active electrode is displaced by a centimeter would surely be extremely small. Hence, we do not think that we are recording retinal currents.

It has been argued that even if the registered responses were directly related to actions of the visual cortex, a more detailed investigation might discover that their actual nature is that of well-known wave phenomena rather than of smooth currents.

One interpretation of this kind is as follows. The α-rhythm can easily be registered from the intact heads of human subjects. But waves of this kind may not appear as waves under all circumstances; because they may be rectified, and then give records which look like records of smoothly flowing currents. This argument does not seem to us to be applicable to our curves. When we use our fast galvanometer, the α-rhythm is readily registered as such with an average amplitude that agrees with the findings of other experimenters. It is so registered before an object is shown, but often also during an exposure. In some records, it is true, the amplitude of the recorded waves rapidly decreases when the exposure begins, and when what seems to be a smooth current appears. One might suspect that, in such instances of "blocking," some part of the α-rhythm is somehow rectified, and that in this fashion the seemingly smooth flow originates. However, it would be hard to apply this reasoning to records in which the α-rhythm which has survived visual stimulation during the pre-exposure period is also not affected by the appearance of the experimental object. In the latter case, the rhythm is superimposed upon the various stages of the smooth flow with about the same amplitude as it had before the smooth deflection began. We do not see how, in such cases, the smooth deflection could be interpreted as a rectified version of α-waves.

In a further explanation, the factor made responsible for the smooth deflection is actually related to cortical visual function. It is assumed that, when an object moves across the field, its appearance in one position after another causes a succession of "on"-effects, each of which resembles the "on"-effect registered upon short stimulation in a given place. In the continuous displacement of the objects used in our experiments, such "on"-effects would overlap, and would thus fuse into the smooth deflections shown in our records. Our answer is that a satisfactory interpretation of our records must explain them all, including those which were taken when the object did not move. Do "on"-effects follow one another for periods of many seconds when a stationary object is shown? Moreover, we have also recorded auditory responses, and when they were taken, the

stimuli were tones, the frequency of which did not vary in a given experiment. Under such conditions, cortical excitation is not supposed to shift from one place to another. Nevertheless, the smooth component of the auditory response often persists during considerable periods of stimulation. We have, for instance, taken one record from the brain of a cat, in which the smooth response to a tone of 5,500 cycles persists for almost 12 sec. We cannot agree with the view that we are registering fused "on"-effects until we are told how the explanation is to be applied to such facts.

To many, the following assumption would appear to be particularly attractive. Synchronized nerve impulses which arrive in one place of the cortex after another while an object moves across the field would not be registered as such by our instruments. Rather, they would fuse in our records, and would thus assume the form of a smooth flow. Actually, however, there would be no such flow in the brain. Those who favor this view will have to admit that it leads to somewhat strange consequences. Is there any evidence that nerve impulses establish potentials of the required size on the surface of the head? Also, would it not be surprising if, after long but unsuccessful work with amplifying instruments especially adapted to the task, the missing evidence had now been found just when a less discriminating amplifier was used? For this we would have achieved according to the present interpretation. The fact that, as obtained with our instruments, the discovery would be expressed in a somewhat inadequate language would not, of course, be very important, since those who accept the interpretation could now, by using their own instruments, at once reduce the blurred message to its true meaning in terms of synchronized nerve impulses. Another question refers to the fact that dark objects cause the same responses as bright objects. We admit that some neurons may be thrown into action when in a given part of the retina stimulation is suddenly replaced by darkness. But, so far as we know, it has never been suggested that impulse action in a dark region may become stronger than such action is in the bright environment of the region. And yet, circumscribed dark areas establish responses which, on the average, seem to be about as strong as those caused by bright objects. Moreover, such responses are also maximal when the dark objects pass the active electrode.

None the less, an explanation of our records in terms of synchronized nerve impulses may not be entirely impossible, provided certain further implications are fully recognized and accepted. Generally, the view now under discussion is formulated in a way which suggests that, if this view were correct, brain physiology would be just where it was before such records were obtained. We believe that this suggestion is mistaken. In the first place, if it were the true meaning of our records that synchronized nerve impulses accompany the perception of visual objects, we should at last have learned what happens in the brain when certain mental facts occur. If it be objected that about this phase of cortical function there has never been any doubt among theorists, our answer is, of course, that the mere arrival of nerve impulses in the visual cortex cannot, as such, explain our findings. Let us assume for a moment, that the registered responses actually consist of synchronized nerve impulses. Then these impulses would behave in a way which is contrary to opinions widely held in neurophysiology. The impulses would announce their occurrence at very great distances from their fibers, because the currents of our records begin to appear when their source is still far removed from the active electrode. This could happen only if the current of a nerve impulse were not restricted to the immediate neighborhood of its fiber, but spread far into the surrounding tissue. Naturally, this is also the only way in which nerve impulses could establish measurable potentials on the surface of the head. Those who believe that the present interpretation covers all our facts do not always seem to realize that it would force neurophysiologists to change their concept of a nerve impulse quite radically. How precisely would the currents of synchronized nerve impulses have to be distributed when they spread through brain, skull, and scalp? As Gerard once explained when discussing a slightly different situation, the flow which issues from a given neuron could not generally return to this neuron on a short path. For if other impulses arrive strictly at the same time in fibers of the immediate neighborhood, virtually any flow from the neuron through this neighborhood would be balanced by electromotive forces which operate in the opposite direction. Thus, when synchronized nerve impulses arrive in the area of a bright object, their currents would circle around the boundary between this area and its darker environment; the currents which issue from the neurons in the middle of the area would

envelop the flow of all other neurons in this area; and the former currents alone would affect the active electrode on the surface of the head. It is, we hope, perfectly clear what this means. If the present explanation were accepted, the currents of synchronized nerve impulses would have to be distributed in about the same way as the smooth flow which our records show when taken at their face value.

The responses to dark objects would have to be understood in the same fashion. If nerve impulses are actually currents which spread through large parts of the brain, then their flow must be strongly concentrated in circumscribed parts in which impulse action is weaker; for through such parts a short path leads back to the source. This is, of course, the beginning of the same explanation as we have given above on the premise that the flow is really smooth. Needless to say, only from this point of view would it also be clear why electrotonic action appears to be stronger within circumscribed dark areas than it is in large bright fields.

All this is, of course, speculation which we hesitate to accept. At any rate, the question whether nerve impulses can actually behave in this fashion will have to be answered by physiologists who know the impulses better than we do.

In a third group of explanations, the smooth flow which seems to be registered in our records is accepted as such, and the explanations refer only to the question what electromotive forces drive the current through the tissue.

One possibility would be that the forces are those of thoroughly non-synchronized nerve impulses. Suppose that in a certain area of the visual cortex the frequency and the density of arriving impulses are about constant from point to point, and that the temporal relations among these impulses are random. In any part of this area which, although small, is large enough to receive many impulses, a certain potential would then be kept statistically constant, even though the location of elements active at a given moment would slightly fluctuate within the small part. The same would be true of all the parts which together make up the area under consideration, so that in the area as a whole a practically steady potential would be maintained. If the area corresponds to a bright object which is surrounded by a dark environment, this steady voltage would drive a current through the area and around its boundary. It seems that, as a matter of principle, all facts found in our investigation could be derived from this assumption. Here again, however, the changed concept of nerve impulses, which we have just discussed, would have to be adopted. Moreover, any interpretation of our records in terms of nerve impulses is confronted with a certain difficulty inasmuch as visual currents and auditory responses have opposite polarities. To be sure, this difficulty would disappear if it could be proved that active elements in the two areas have correspondingly different orientations.

To Dr. Lorente de Nó we owe the following suggestion. We have found that the investigated currents establish electrotonic effects. But it is also possible that the currents themselves are caused by effects of this kind, namely, by electrotonic potentials which nerve impulses establish in their neurons. The flow maintained by such voltages would have the distribution which we have ascribed to the currents shown in our records. This seems to us a plausible theory. We hope that Dr. Lorente de Nó will soon formulate it more explicitly.

A last explanation which we have to consider refers to the chemical changes which, according to some physiologists, arise in the environment of cortical cells when nerve impulses arrive in this environment. If it were true that nerve impulses change ionic concentrations on the surfaces of living membranes, and that, as a result, electromotive forces are here established, such forces would drive currents through and around the areas in which this happens. Voltages which originate when membranes are in contact with certain electrolytes have often been investigated.[23] The polarity of membrane potentials in this sense depends both on the specific nature of the membranes and on that of the ions in question. Consequently, the fact that visual and auditory currents flow in opposite directions could be understood if, in one respect or the other, there were a corresponding difference between the auditory and the visual situation. No special asumption of this kind would be needed if the opposite polarities of visual and auditory currents were caused by different orientations of the active elements in the two areas.

[23] *Cf.* R. Höber, Physical chemistry of cells and tissues, 313–333, The Blakiston Co., 1948.

To summarize: In all explanations which seem to be compatible with our findings, it is assumed that currents flow through cortical objects, and return to these objects through their environment. If the acting voltages were those of synchronized nerve impulses, these currents would pulsate with the frequency of the impulses; but their distribution in the tissue would be approximately the same as that of a smooth flow which is maintained by a more permanent source. According to other theories, the driving forces are actually fairly constant, and thus the resulting currents are coherent also in time. From the point of view of psychology, the distribution of the currents in cortical space is the most important issue. Fortunately, we have seen, all theories which are otherwise acceptable lead to about the same consequences in this respect. Under the circumstances, attempts to study the relations between the behavior of cortical currents and the main characteristics of visual space may perhaps be made with some confidence. So far the concept of visual currents has been used in explaining only the segregation of molar visual objects, and the fact that, irrespective of their brightness, such objects play the role of areas in which cortical energy is highly concentrated. But molar objects have many specific properties, and their appearance in a visual field has equally specific biological consequences. We are inclined to believe that a theory of the objects as such must also be applicable to these further problems.

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THE TWO HUNDREDTH ANNIVERSARY OF BENJAMIN FRANKLIN'S TWO LIGHTNING EXPERIMENTS AND THE INTRODUCTION OF THE LIGHTNING ROD*

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(*Read April 25, 1952*)

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1. INTRODUCTION

Two hundred years have elapsed since the first experiments indicated that the lightning discharge is an electrical phenomenon. Books and articles—on American history, on Franklin, physics, and the history of science—exhibit so much uncertainty and even confusion about this subject that the bicentenary of the performance of these experiments may fittingly be marked by an attempt to bring together the relevant printed and manuscript documents and to ascertain, in so far as possible, the sequence of events in Franklin's research on lightning in 1752.

The one fact about Benjamin Franklin's scientific career that is known to every reader of American history is that he once flew a kite during a thunder-storm; yet I am sure that most Americans would find extreme difficulty in answering the questions of precisely why he flew the kite and exactly what he learned in the process. Depicted in a famous Currier and Ives print (see fig. 1), the familiar picture of Franklin raising his kite during a thunder-storm has become dear to generation after generation, while his magnificent contributions—both theoretical and experimental—to the budding science of electricity, which gained him in his lifetime the adulatory title of the "Newton" of the subject, have been ignored or forgotten.[1] Hence, it is not particularly surprising that the significance of the kite experiment in terms of the development and acceptance of Franklin's ideas about electricitiy, or even lightning, has gradually become lost. Even those physicists who can readily understand what the experiment proved are apt to add such a statement as :"Perhaps the most wonderful part of it was that Franklin was not killed at once."[2]

A statement has been published in the *Proceedings* of this Society that the kite experiment was never performed at all:

> You have all heard the story of his kite-experiment, in which he [Franklin] got electric sparks between a kite-string and a key while the kite was flying in or near a thunder cloud. I regret to have to inform you that in the opinion of local historians this is just a myth, one of those legends which spring up from unknown sources to adorn the story of a great man.[3]

Rather than a myth or legend springing from unknown sources, the account of the kite experiment derives directly from Franklin himself. Even though there may be uncertainty about the actual day on which the kite was first flown, I do not believe that the performance of the experiment as such is subject to legitimate doubt.

* This article was written with the assistance of a grant from the Penrose Fund of the American Philosophical Society.

[1] The writer is at present completing a monograph on this subject which will be published by the American Philosophical Society. A preliminary report has appeared in the American Philosophical Society *Year Book* for 1949: 240–243, 1950; see also Benjamin Franklin, an experimental Newtonian scientist, *Bull. Amer. Acad. Arts and Sci.* **5** (4) : 2–6, 1952.

[2] Black, N. H., *An introductory course in college physics,* 388, N. Y., Macmillan, ed. 3, 1948.

[3] *Proc. Amer. Philos. Soc.* **91**: 17, 1947. Note that Franklin did not get "sparks between a kite-string and a key," but rather between the key and his knuckle.

FRANKLIN'S EXPERIMENT, JUNE 1752.
Demonstrating the identity of Lightning and Electricity, from which he invented the Lightning Rod.

FIG. 1. Franklin and the Kite, as represented by Currier and Ives. This "stock" representation of the great experiment contains one important error which is commonly found in pictures of the kite experiment. Franklin's son is shown as a young boy, although he was at least twenty-one years of age; about two years before the kite experiment, i.e., on 12 April 1750, Franklin had written to his mother about family affairs, noting: "As to your Grandchildren, Will is now nineteen years of age. . . ." Furthermore, Franklin grasps the string above the key, he is not sheltered, and he is near a house, etc. (Reproduced from the copy belonging to Mr. Ernest A. Hale of the Suffolk First Federal Savings & Loan Association, Franklin Street, Boston.)

In the following pages, I will indicate the evidence that the kite experiment was performed: not only by Franklin but by others. It may come as a surprise, however, to learn that this experiment had been independently conceived by a French experimenter, Romas. Actually, the electrical kite was the second test instrument that Franklin had devised to investigate the electrification of clouds; the earlier one was the insulated rod in that "sentry-box" experiment which had been brought to a successful issue even before Franklin raised his kite and which would have rendered the kite experiment unnecessary, had the news reached Franklin in time. I believe that the evidence in the following pages will fully support Franklin's statement that the kite experiment was performed before he had heard of the success in Europe of his sentry-box experiment. We may conclude that the kite experiment was performed by Franklin in June 1752, and that the first grounded lightning rods were introduced to the world in Philadelphia in 1752 by Benjamin Franklin—probably in June, but possibly in July.

2. FRANKLIN'S KITE EXPERIMENT

The letter in which Franklin described his kite experiment was written from Philadelphia on 1 October 1752. It was addressed to Peter Collin-

son, F.R.S., who had earlier provided Franklin with some simple apparatus for performing electrical experiments; many of Franklin's earliest contributions were addressed to Collinson.[4] A copy of the original letter, at present in the archives of the Royal Society of London, is reproduced as figure 2. It reads as follows:[5]

From Ben[a]: Franklin, Esq. Philadelphia
To P. Collinson Octo: 1: 1752

As Frequent mention is made in the Publick papers from Europe, of the Success of the *Philadelphia-Experiment* for drawing the Electrick Fire from Clouds by means of Pointed Rods of Iron erected on high Buildings &c., it may be agreeable to the Curious to be informed, that the same Experim[t]. has succeeded in Philadelphia Tho' made in a Different & more Easie manner, which any one may try as follows:

Make a small Cross of Two light Strips of Cedar, the arms so long as to reach to the four Corners of a Large Thin Silk Handkerchief, when extended; Tie the corners of the handkerchief to the extremities of the Cross; So you have the Body of a Kite, which being properly accomodated with a Tail, Loop, & String, will rise in the Air like those made of paper; but this being of Silk is fitter to bear the Wett & Wind of a Thunder Gust without Tearing.

To the Top of the upright Stick of the Cross is to be fixed a very Sharp pointed Wire, riseing a foot or more above the Wood.

To the end of the Twine, next the hand, is to be tied a Silk Ribon; and where the Twine & Silk joyn, a Key may be fasten'd.

The Kite is to be raised, when a Thunder Gust appears to be comeing on (which is very frequent in this Country) & The Person who holds the String, must stand within a Door, or Window, or under some cover, so that the Silk Ribon may not be Wet; & care must be taken, that the Twine does not touch the Frame of the Door or Window.

As soone as any of the Thunder Clouds come over the Kite, the pointed wire will draw the Electric Fire from them, & the Kite, with all the Twine, will be Electrified and the loose filaments of the Twine will stand out every way, and be attracted by an approaching finger.

When the Rain has Wett the Kite & Twine, so that it can conduct the Electric Fire freely, you will find it stream out plentifully from the Key on the approach of your Knuckle.

At this key the Phial may be Charged, and from Electric Fire thus obtained, Spirits may be kindled, and all the Other Electrical Experim[ts] be performed, which are usually done by the help of a rubbed Glass Globe or Tube, & thereby the Sameness of the Electric Matter with that of Lightning compleatly demonstrated.

I was pleased to hear of the Success of My experiments in France, & that they there begin to Erect points on their buildings. We had before placed them upon our Academy & State House Spires.

This letter was read at the Royal Society on 21 December 1752 and was published in the Society's *Philosophical Transactions* for 1751 and 1752. It was also printed in the *Gentleman's Magazine* and the *London Magazine* and was included in the second part of Franklin's book on electricity. In America it was published in Franklin's *Pennsylvania Gazette,* and reprinted in the Boston *Gazette* and in other publications.[6]

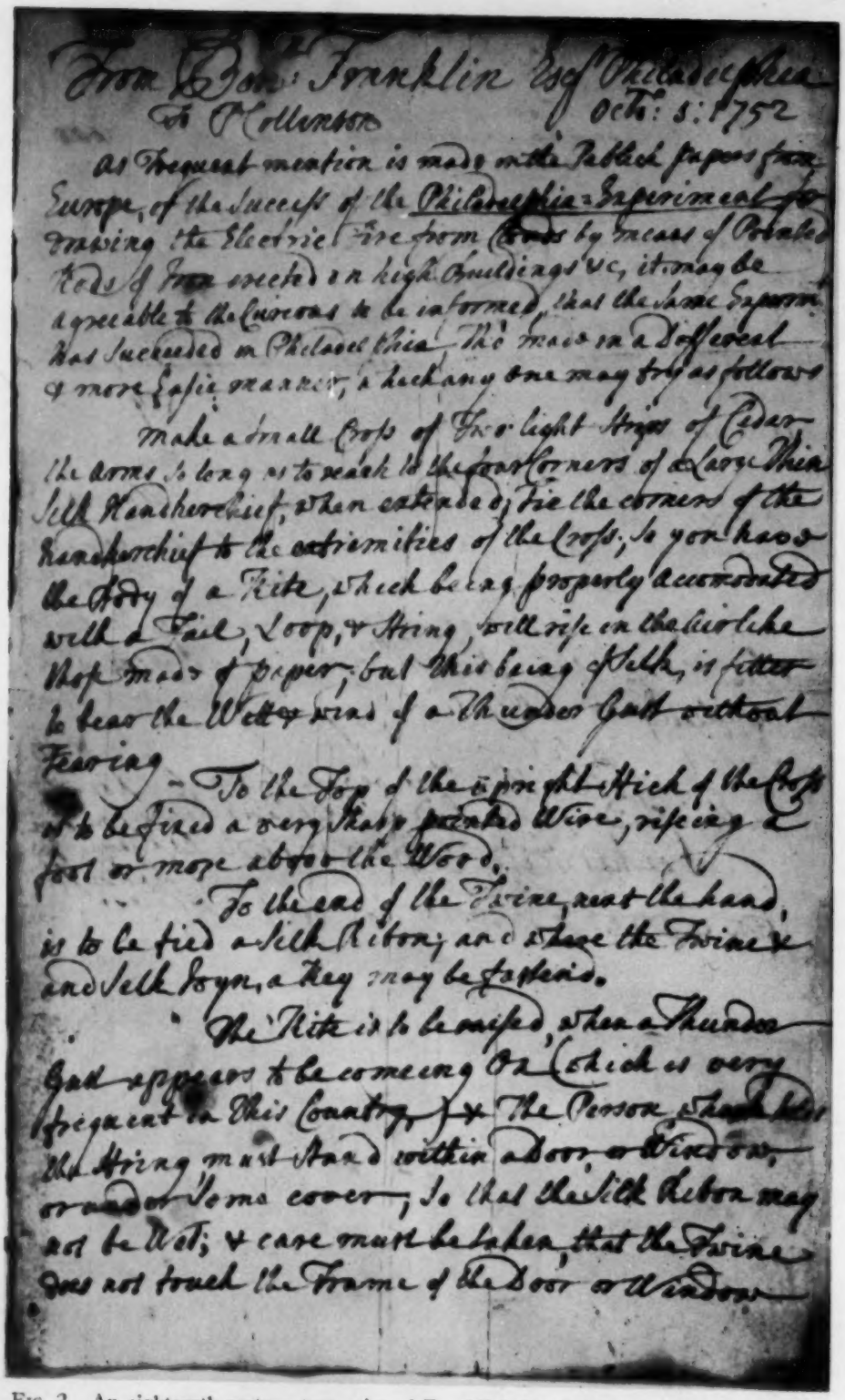
A further and corroborative source of information is provided by Joseph Priestley's *History and present state of electricity.* This book has a special value, as Jernegan has shown,[7] since Franklin was in close contact with Priestley and undertook to supply Priestley with the books he needed. Furthermore, Priestley wrote that he had "kept up a constant correspondence with Dr. Franklin, and the rest of my philosophical friends in London;

[4] *Cf.* Cohen, I. Bernard, *Benjamin Franklin's experiments: a new edition of Franklin's "Experiments and observations on electricity," edited, with a critical and historical introduction,* intro., ch. III, pt. 1, Cambridge, Harvard Univ. Press, 1941; also Brett-James, Norman G., *The life of Peter Collinson* [London], The Friends' Bookshop [no date].

[5] This letter is reproduced with the kind permission of the Royal Society of London.

[6] The text of this letter in *Phil. Trans. Roy. Soc.* **47**: 565–566, 1751 and 1752 is identical, word for word, with the manuscript copy and follows the same paragraphing. The only difference between the two is in punctuation and spelling, e.g., "Publick" [in the MS] for "public" [in the *Phil. Trans.*], "*Philadelphia-Experiment*" for "Philadelphia experiment," "Electrick" for "electric," "easie" for "easy," "Ribon" for "riband," "joyn" for "join," "soone" for "soon," "compleatly" for "completely," "riseing" for "rising," "comeing" for "coming," and generally, "&" for "and." When printed in the *Pennsylvania Gazette,* the *Gentleman's Magazine, London Magazine,* and the several editions of his book on electricity, the letter bore the date of 19 October 1752 rather than 1 October 1752, and the final paragraph of the letter was missing. On this score, see, *infra,* § 5, and especially footnote 50.

[7] Jernegan, Marcus W., Benjamin Franklin's "electrical kite" and lightning rod, *New England Quart.* **1**: 180–196, 1928. The value of Priestley's testimony had been recognized earlier by James Parton (*cf. Life and times of Benjamin Franklin* **1**: 295, N. Y., Mason Brothers, 1864), who, in turn, had obtained his information from Sparks. The latter had prefaced the section of his edition containing the electrical papers by a pair of long extracts from Priestley and Stuber (Sparks, Jared, *The works of Benjamin Franklin* **5**: 173–180, Phila., Childs & Peterson, revised ed., 1840).



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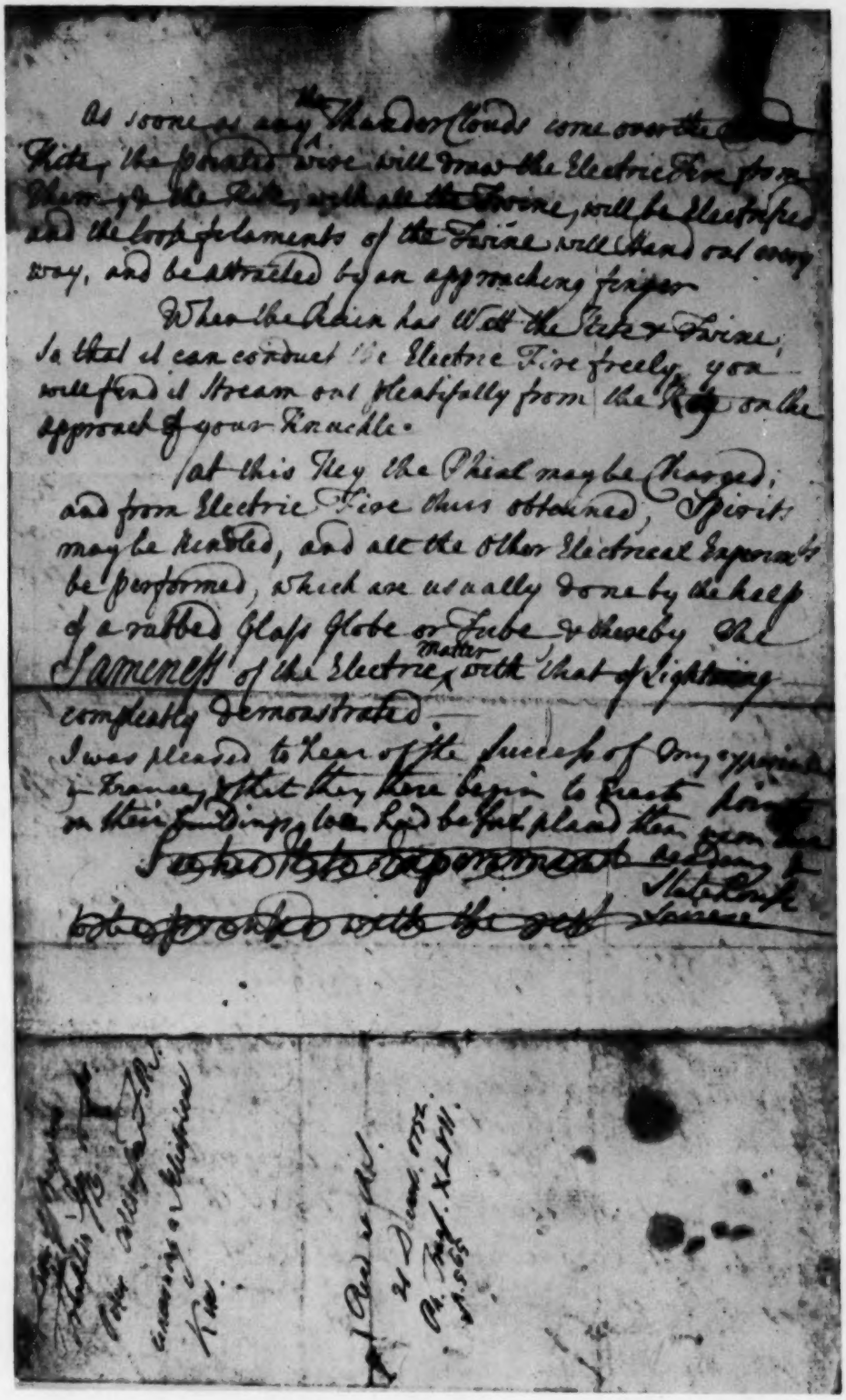
To the end of the String, next the hand, is to be tied a Silk Ribbon; and where the String & and Silk Ribbon join, a Key may be fastened.

The Kite is to be raised, when a Thunder Gust appears to be coming on, (which is very frequent in this Country) & the Person who holds the String must stand within a Door or Window, or some cover, so that the Silk Ribbon may not be Wet; & Care must be taken, that the String does not touch the Frame of the Door or Window

FIG. 2. An eighteenth-century transcript of Franklin's kite letter; the original is in the archives of the Royal Society of London and is reproduced by permission. This copy appears to be in the hand of Peter Collinson and, although the spelling of several words differs from the text printed in the *Philosophical Transactions*, the date is the



same, and the paragraphing is the same as the version published in the *Philosophical Transactions;* it should also be noted that the final paragraph about "points" erected in Philadelphia, is also included in this version.

and my letters circulated among them all, as also every part of my history as it was transcribed." [8] Priestley's description appears, therefore, to have been based on information provided by Franklin himself and, since Franklin read the manuscript, must have had the seal of his approval. This account is more detailed than Franklin's letter to Collinson; for this reason Carl Van Doren preferred Priestley's description to Franklin's, and printed it in his edition of *Benjamin Franklin's autobiographical writings,* noting that it "contains precise details about the kite experiment which Priestley could have had only from Franklin, to whom Priestley refers as 'the best authority.'" [9] Priestley's account follows:

> As every circumstance relating to so capital a discovery (the greatest, perhaps, since the time of Sir Isaac Newton) cannot but give pleasure to all my readers, I shall endeavour to gratify them with the communication of a few particulars which I have from the best authority.
>
> The Doctor, having published his method of verifying his hypothesis concerning the sameness of electricity with the matter of lightning, was waiting for the erection of a spire in Philadelphia to carry his views into execution, not imagining that a pointed rod of a moderate height could answer the purpose, when it occurred to him that by means of a common kite he could have better access to the regions of thunder than by any spire whatever. Preparing, therefore, a large silk handkerchief and two cross-sticks of a proper length on which to extend it, he took the opportunity of the first approaching thunder-storm to take a walk in the fields, in which there was a shed convenient for his purpose. But, dreading the ridicule which too commonly attends unsuccessful attempts in science, he communicated his intended experiment to nobody but his son who assisted him in raising the kite.
>
> The kite being raised, a considerable time elapsed before there was any appearance of its being electrified. One very promising cloud had passed over it without any effect, when, at length, just as he was beginning to despair of his contrivance, he observed some loose threads of the hempen string to stand erect and to avoid one another, just as if they had been suspended on a common conductor. Struck with this promising appearance, he immediately presented his knuckle to the key, and (let the reader judge of the exquisite pleasure he must have felt at that moment) the discovery was complete. He perceived a very evident electric spark. Others succeeded, even before the string was wet, so as to put the matter past all dispute, and when the rain had wet the string he collected electric fire very copiously. This happened in June 1752, a month after the electricians in France had verified the same theory, but before he heard of anything they had done.[10]

On the basis of Priestley's report, we have further reason to believe that Benjamin Franklin performed the experiment as he described it in his letter to Collinson, and that the date was June 1752.

Stripped to its barest essentials, the kite experiment employed a long insulated conductor, terminating in a point at its uppermost end which was raised high above the ground; when a thunder-cloud passed overhead, Franklin momentarily grounded the insulated conductor by bringing his knuckle to the key, whereupon a spark passed between his knuckle and the key. The similarity between this experiment and the familiar experiments of the laboratory showed that thunder-clouds are electrostatically charged; hence their discharge must be an electrical discharge, differing in scale, but not in kind, from those produced in the laboratory.[11]

[8] From a letter from Priestley to Rotheram, dated 1-5 February 1766, quoted in Jernegan (ref. 7, *supra*), 187. In the preface to his history, Priestley wrote: "With gratitude I acknowledge my obligations to Dr. Watson, Dr. Franklin, and Mr. Canton, for the books, and other materials with which they have supplied me, and for the readiness with which they have given me any information in their power to procure," Priestley, Joseph, *The history and present state of electricity, with original experiments* **1**: xi, London, C. Bathurst and T. Lowndes *et al.*, ed. 3 "corrected and enlarged," 1775. *Cf.* Walker, W. Cameron, The beginnings of the scientific career of Joseph Priestley, *Isis* **21**: 81–97, 1934, esp. pp. 87, 89.

[9] Van Doren, Carl, *Benjamin Franklin's autobiographical writings,* 76, N. Y., Viking, 1945.

[10] Priestley, History (ref. 8, *supra*) **1**: 216–217. This account is preceded by a résumé of Franklin's letter to Collinson: "To demonstrate, in the completest manner possible, the sameness of the electric fluid with the matter of lightning, Dr. Franklin, astonishing as it must have appeared, contrived actually to bring lightning from the heavens, by means of an electrical kite, which he raised when a storm of thunder was perceived to be coming on. This kite had a pointed wire fixed upon it, by which it drew the lightning from the clouds. This lightning descended by the hempen string, and was received by a key tied to the extremity of it; that part of the string which was held in his hand being of silk, that the electric virtue might stop when it came to the key. He found that the string would conduct electricity even when nearly dry, but that when it was wet, it could conduct it quite freely; so that it would stream out plentifully from the key, at the approach of a person's finger. At this key he charged phials, and from electric fire thus obtained, he kindled spirits, and performed all other electrical experiments which are usually exhibited by an excited globe or tube."

[11] For a discussion of what actually is believed to occur in such an experiment, see, *infra,* fig. 5, and the material in § 6 above footnotes 61–64; also footnote 98.

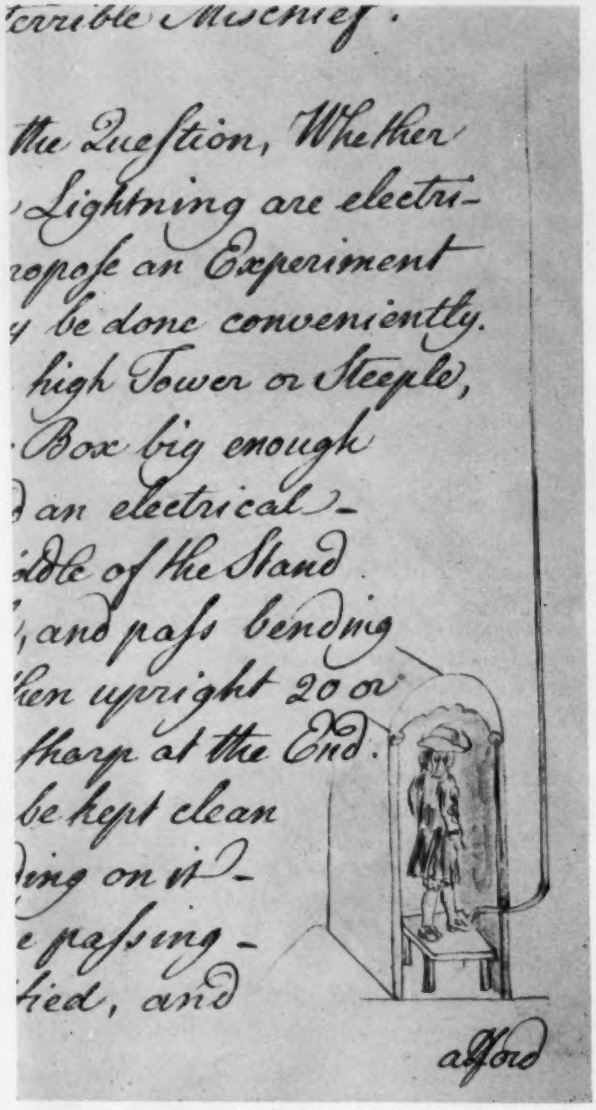


FIG. 3. Franklin's sentry-box experiment. This drawing is reproduced from the "Bowdoin MS" [12]—the drawing was made under Franklin's supervision, probably not by Franklin himself. (Reproduced with the permission of the American Academy of Arts and Sciences.)

[12] The "Bowdoin MS" was discovered by the writer during the preparation of *Benjamin Franklin's experiments* (ref. 4, *supra*) and is described on pages 152–154 of that work. It consists of a manuscript copy of all of Franklin's earliest papers on electricity, copied by two amanuenses and corrected by Franklin, and sent by Franklin to Bowdoin in 1750. As such it may be considered to replace the original MS copies of the letters, which no longer exist. In a number of instances, the "Bowdoin MS" gives a complete text, names of addressees, and dates, which are not to be found in either the printed editions of Franklin's book on electricity, or the collected editions of his writings; the text in *Benjamin Franklin's experiments* has been annotated to include the corrections made necessary by the "Bowdoin MS."

3. FRANKLIN'S SENTRY-BOX EXPERIMENT

The original experiment designed by Franklin to test the hypothesis that lightning is an electrical discharge between clouds, or between clouds and the earth, occurs in a paper entitled "Opinions and conjectures, concerning the properties and effects of the electrical matter, arising from experiments and observations, made at Philadelphia, 1749," and enclosed in a letter written by Franklin to Peter Collinson from Philadelphia, 29 July 1750. It reads as follows:

> To determine the question, whether the clouds that contain lightning are electrified or not, I would propose an experiment to be tried where it may be done conveniently. On the top of some high tower or steeple, place a kind of sentry-box big enough to contain a man and an electrical stand. From the middle of the stand let an iron rod rise and pass bending out of the door, and then upright 20 or 30 feet, pointed very sharp at the end. If the electrical stand be kept clean and dry, a man standing on it when such clouds are passing low, might be electrified and afford sparks, the rod drawing fire to him from a cloud. If any danger to the man should be apprehended (though I think there would be none) let him stand on the floor of his box, and now and then bring near to the rod the loop of a wire that has one end fastened to the leads, he holding it by a wax handle; so the sparks, if the rod is electrified, will strike from the rod to the wire, and not affect him.[13] [See fig. 3.]

It will be noted that if this experiment be stripped to its essentials, it is identical to the experiment of the kite; here, too, a pointed, insulated conductor is used to indicate whether thunderclouds overhead are electrically charged. In fact, the only real difference between the two experiments is that in the case of the kite the upper end of the pointed, insulated conductor is much higher in the air than the 20 or 30 feet above "some high tower or steeple" recommended by Franklin for the sentry-box experiment.

Franklin's description of the sentry-box experiment was published in his book on electricity, *Experiments and Observations on Electricity, made at Philadelphia in America,* issued in London in 1751.[14] A French translation, made by Jean François Dalibard (or d'Alibard) at the request

[13] *Benjamin Franklin's experiments* (ref. 4, *supra*), 222.

[14] A description of the various editions of Franklin's book on electricity may be found in *Benjamin Franklin's experiments* (ref. 4, *supra*).

of Buffon, was published in France in 1752.[15] This book created something of a sensation.

> The Philadelphian experiments . . . having been universally admired in France, the King desired to see them performed. . . . His Majesty saw them with great satisfaction, and greatly applauded Messieurs Franklin and Collinson. These applauses of his Majesty having excited in Messieurs de Buffon, D'Alibard and de Lor, a desire of verifying the conjectures of Mr. Franklin, upon the analogy of thunder and electricity, they prepar'd themselves for making the experiment.[16]

The above description is taken from the report of the Abbé Mazéas, in a letter to Stephen Hales dated 20 May 1752 and read at the Royal Society on 28 May 1752. Mazéas then described Dalibard's experiments, in which the insulated rod had been erected in a sentry box in a garden at Marly-la-ville; the "pointed bar of iron" was "40 feet high." During Dalibard's absence from Marly, the apparatus was entrusted to an "ancien dragon" named Coiffier. "On the 10 of May, 20 minutes past 2 afternoon, a stormy cloud having passed over the place where the bar stood, those, that were appointed to observe it [i.e., Coiffier and the village priest, Raulet], drew near, and attracted from it sparks of fire, perceiving the same kind of commotions as in the common Electrical experiments." Word was sent to Dalibard who, on 13 May 1752, read a detailed account of the experiment, and the Franklinian principles of electricity it illustrated, to the members of the Académie Royale des Sciences in Paris.[17] On 18 May Delor repeated the experiment with similar success, using "a bar of iron 99 feet high, placed upon a cake of resin, two feet square, and 3 inches thick."[18] Before long, others had repeated the experiment, Le Monnier, Abbé Nollet, and others, in France, Mylius and Ludolf in Germany, and, eventually, Canton, Wilson, and Bevis in England.[19]

If we accept Priestley's statement that the kite was flown in June 1752, then the Marly experiment of May 1752 antedated it by one month. This is perfectly consistent with Franklin's statement in the autobiography, when, after referring to the "capital [Marly] Experiment," he mentioned "the infinite Pleasure I receiv'd in the Success of a similar one I made soon after with a Kite in Philadelphia."[20] In 1768 Franklin wrote Dalibard a note in which he admitted freely that Dalibard was "the first of Mankind, that had the Courage to attempt drawing Lightning from the Clouds to be

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[15] In his autobiography Franklin wrote that "A Copy of them [my electrical papers] happening to fall into the Hands of the Count de Buffon . . . he prevail'd with M. Dalibard to translate them into French, and they were printed at Paris." From Farrand, Max, *Benjamin Franklin's memoirs, parallel text edition, comprising the texts of Franklin's original manuscript, the French translation by Louis Guillaume le Veillard, the French translation published by Buisson, and the version edited by William Temple Franklin, his grandson, edited with an introduction and explanatory notes*, 384–385 ["original MS"], Berkeley and Los Angeles, Univ. of Calif. Press, 1949.

Dalibard, in the "avertissement" to his translation, noted that Franklin "pria en même tems M. Collinson d'en envoyer un des premiers exemplaires à M. de Buffon . . . [qui] m'a engagé à les faire paroître en François." (First French edition [F1], p. 4, second French edition [F2.1], pp. 2–3; *cf.* note 14, *supra.*)

The Abbé Nollet, in *Lettres sur l'électricité* **1**: 5, Paris, Chez H. L. Guérin & L. F. Delatour, 1754 (*ed. princeps* 1752), a book devoted largely to an attack on Franklin's experiments and theories, noted that soon after the appearance of the English edition of Franklin's book (1751), "Un particulier qui reçut cet Ouvrage à Paris, le traduisit en Français pour son propre usage, dit-on, & sans avoir dessein de le faire imprimer; cela est d'autant plus vraisemblable que cette traduction est un peu négligée; dans bien des endroits on a peine à entendre l'Auteur, & l'on manqueroit plusiers de ses expériences, si l'on n'avoit pas recours à l'Original Anglois.

"Quoi qu'il en soit, cet Ouvrage traduit dans notre langue, tomba entre les mains de M. de Buffon, Intendant du Jardin du Roi. Ce savant Académicien, goûtant beaucoup la doctrine de M. Franklin, & ayant répété & vérifié, à ce que l'on prétend, avec succès toutes les expériences que cet Auteur rapporte en preuves, crut obliger sa Patrie, en faisant publier cette traduction; & comme il étoit livré à des occupations plus importantes, il en abandonna le soin à un de ses amis nommé M. Dalibard, qui y joignit de son chef une histoire abrégée de l'Electricité; & cela forme un petit volume *in*-12. . . ."

[16] *Phil. Trans. Roy. Soc.* **47**: 534–535, 1751–1752.

[17] Dalibard published this memoir in full in the second edition of his translation of Franklin's book (1756 [F2.2; *cf.* note 14, *supra*], **2**: 67–125) and the major portion of it was included in the fourth and fifth English editions of his book (1769 [E4] and 1774 [E5]); *cf. Benjamin Franklin's experiments* (ref. 4, *supra*), 257–262.

[18] *Phil Trans. Roy. Soc.* **47**: 535, 1751–1752.

[19] Accounts of these experiments were printed in *Phil. Trans. Roy. Soc.* **47**: 536–552 [experiments of Mazéas, Le Monnier, and (p. 550) of Ludolf as reported by Euler], 557–558 [expts. of Nollet, and Le Roy], 559 [expts. of Mylius and Ludolf], 567–570 [expts. of Watson, Canton, Wilson, and Bevis]. These experiments are discussed in more detail in two articles by the writer, Prejudices against lightning rods, *Jour. Franklin Inst.* (in press), and Franklin & Diviš, *Isis* (in press). Another who repeated the sentry-box experiment in France was Romas, who claimed that he had conceived this experiment long before Franklin, just as he claimed that he had anticipated the kite experiment; see, *infra*, § 8.

[20] *Benjamin Franklin's memoirs* (ref. 15, *supra*), 386.

subjected to your experiments." [21] We must next investigate the question of whether, when Franklin flew his kite, he had already learned of the European experiments performed according to the Franklin specifications of 1750.

4. WHEN DID FRANKLIN LEARN OF THE EUROPEAN SENTRY-BOX EXPERIMENTS?

The question of the date when Franklin received information about the successful issue of the sentry-box experiments performed in Europe has been studied by Abbott Lawrence Rotch,[22] Alexander McAdie,[23] Marcus W. Jernegan,[24] and Carl Van Doren.[25] Jernegan and Van Doren accepted Priestley's date of June 1752 for the kite, so that they endorsed the conclusion that Franklin had flown his kite before he had heard about the French experiments: Jernegan and Van Doren quoted Priestley's description of the kite experiment which I have printed above.[26] Priestley included in his history another discussion of this chronology, which reads:

> Moreover, though Dr. Franklin's directions were first begun to be put in execution in France, he himself completed the demonstration of his own problem, before he heard of what had been done elsewhere: and he extended his experiments so far as actually to imitate almost all the known effects of lightning by electricity, and to perform every electrical experiment by lightning.[27]

This statement is even more explicit than the one quoted earlier. Even so, all writers have not been willing to accept Priestley's testimony. Thus Rotch believed that "the experiment was not performed until later in the summer" than June,[28] and McAdie concluded:

> 1: Franklin himself does not give a definite date when a kite was flown.
>
> 2: It seems doubtful that the kite was flown in June or early summer 1752.
>
> 3: If flown, the date was probably not far in advance of the end of September 1752.[29]

Rotch's and McAdie's conclusions would, according to Jernegan, "take from Franklin a part of the honor which he has had because of the belief that he independently made the discovery, by his own experiment, of the identity of lightning and electricity." His "kite experiment would rather be a continuation of those already performed; an attempt to extend them further." The expression, "by his own experiment," is a little misleading; at the very least, it is ambiguous, since both experiments were Franklin's. Dalibard, in his report to the French Academy of Sciences on 13 May 1752, was explicit on this point: "In following the path that M. Franklin traced for us, I have obtained a complete satisfaction." [30] Furthermore, it is difficult to know in what sense the kite experiment might have been conceived as "an attempt to extend them [i.e., those experiments already performed] further," [31] since the results

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[21] Smyth, A. H., *Writings of Benjamin Franklin* **5**: 94, N. Y., Macmillan, 1907. *Cf., infra,* Priestley's statement printed above footnote 27.

[22] Rotch, A. L., Did Benjamin Franklin fly his electrical kite before he invented his lightning rod? *Proc. Amer. Ant. Soc.* **18**: 115–123, 1907; When did Franklin invent the lightning rod? *Science* **24**: 374–376, 1906; The lightning rod coincided with Franklin's kite experiment, *Science* **24**: 780, 1906.

[23] McAdie, A., The date of Franklin's kite experiment, *Proc. Amer. Ant. Soc.* **34**: 188–205, 1925.

[24] Reference 7, *supra.*

[25] Van Doren, C., *Benjamin Franklin,* 164–170, N. Y., Viking, 1938.

[26] *Cf., supra,* the extract quoted above footnote 10.

[27] Reference 8, *supra,* **1**: 206.

[28] Rotch relied chiefly on the testimony of Romas that Franklin knew of the Marly sentry-box experiment before he flew his kite; he concluded (correctly) that the news would probably not have reached Philadelphia in June 1752. Romas was in error, as may be seen in the discussion below, § 8. *Cf.* footnote 31, *infra.*

Rotch's chief aim in the article in *Proc. Amer. Ant. Soc.,* by his own admission, was to make known Franklin's directions for installing lightning rods, as published in "Poor Richard's almanack" for 1753 which, he maintained, "seems to have escaped the notice of all Franklin's biographers." Had Rotch consulted the best biography then available [Parton (ref. 7, *supra,* **1**: 297n], he would have found this passage about lightning rods reprinted *in extenso.*

[29] Reference 23, *supra*; see, *infra,* § 7 for a further discussion of McAdie's comments.

[30] *Cf.* his translation of Franklin's book (ref. 14, *supra*) [F2.1] **2**: 99. On pp. 72–73, he wrote, "There was lacking to that ingenious physicist [Franklin] but one last proof in order to produce complete conviction that the matter of thunder is absolutely the same as that of electricity; not being apparently too ready to acquire this proof by himself, he has shown us the means of obtaining it." (Freely translated from the French.)

[31] On this point Jernegan appears to have followed Benjamin, Park, *A history of electricity (the intellectual rise in electricity) from antiquity to the days of Benjamin Franklin,* 589, N. Y., John Wiley, 1898. Benjamin assumed that Franklin had heard about the French experiment before he flew his kite; that he had believed the rod of Dalibard was not high enough [it was far from being as high as Franklin had specified] to draw electrical fluid from the clouds themselves. "That sparks had been drawn from rods which ended in the air close to the earth's surface, and not within hundreds of feet of the

it provided were identical to those of the sentry-box experiment.

After reviewing the evidence, and especially on the basis of new information which he had uncovered, Jernegan concluded:

> Benjamin Franklin proposed the identity of lightning and electricity but by his own admission, a French scientist, M. D'Alibard, was the first to prove his conjectures by "drawing lightning from the clouds to be subjected to your experiments." On the other hand, the evidence presented makes it more certain that Franklin did prove the identity of lightning and electricity, *independently*, and that his "electrical kite" was flown in June, 1752, before he had heard of the French experiments of May 10 and 18. Secondly, while Franklin was the first to propose lightning rods, and to give definite directions for erecting them, the evidence indicates that French scientists, acting on his suggestions, set up "sharp-pointed iron bars" on buildings and ships before May 26, 1752, before Franklin flew his kite, and before he himself had proved by experiment that they were a "preservative against thunder."

As I shall indicate below, there is still further evidence to support Jernegan's acceptance of Franklin's statement to Priestley that the kite experiment had been performed in June 1752 and, therefore, prior to his having received intelligence of the French experiments. On the other hand, I cannot agree that there is any good evidence that lightning rods [32] had been erected in France earlier than those Franklin appears to have erected in Philadelphia in June 1752, and I shall discuss this question at length in the following section.

Jernegan laid stress on a letter written by Franklin to Cadwallader Colden on 14 September 1752, in which he observed, "I see by Cave's *Magazine* for May that they have translated my electrical papers into French and printed them in Paris," [33] since this issue of the *Gentleman's Magazine* contained a letter from France describing experiments "in pursuance of those by Mr. *Franklin* . . . to find whether the tonitruous and electrical matter be not analagous," and referring to "bars." [34] Jernegan therefore concluded that Franklin knew by 14 September that "French scientists had placed iron rods on buildings and that they were a preservative against thunder." The inference is that Franklin knew about the French experiments two weeks before writing his letter about the kite to Collinson on 1 October; this point is also stressed by Van Doren.[35] A thorough examination of the *Pennsylvania Gazette* for the summer of 1752 has revealed that this information was in Franklin's hands almost a month earlier, that is, by the third or fourth week of August, since the issue for 28 August 1752 reprinted a letter concerning European lightning experiments with insulated rods.[36]

I am willing to accept Romas's computation [37] to show that Franklin probably could not have received news of Dalibard's experiment at Marly-la-Ville in June 1752. Dalibard's report was read in Paris on 12 May and Mazéas's letter to Stephen Hales was dated 20 May and was read at the Royal Society on 28 May. Since it would take a month or more for the news to reach Philadelphia from London, Franklin very likely would not have heard about this matter until early July—possibly the last days of June—at the very earliest, had either Collinson or Dalibard written him at once.

The account which Collinson eventually sent to Franklin was based on the Abbé Mazéas's letter, and reads:

> If any of the thy Friends should take Notice that thy Head is held a little higher up than formerly, let them know; the *Grand Monarch of France* strictly commands the *Abbé Mazéas* to write a Letter in the politest Terms to the Royal Society, to return the King's Thanks and Compliments in an express Manner to *Mr. Franklin* of *Pennsylvania*, for the useful Discoveries in Electricity, and Application of the

clouds was not conclusive. This was the experiment in one sense, and yet, in another, it was not. It showed that the rods had become electrified—but not necessarily that the lightning had electrified them or had passed over them." Benjamin believed that the kite experiment was designed to answer the doubt so aroused, but there is no evidence whatever to support his view. We do know that Franklin believed that pointed conductors could "draw off" electrical fluid from charged bodies at great distances and, indeed, this was the basis of his theory of the action of lightning rods. If he had overestimated the height necessary for a rod to "draw off" the supposed electrical fire from the clouds, I cannot see why this should have necessitated another experiment. We know that by September 1752, when he had certainly known about the French experiments, he tested the electrification of clouds by the use of a rod he erected on his house in Philadelphia; *cf.*, *infra*, § 6.

[32] I.e., in the sense of grounded conductors to prevent buildings from damage by a stroke of lightning; *cf.*, *infra*, § 6.

[33] Smyth, Albert Henry, *The writings of Benjamin Franklin* **3**: 98, N. Y., Macmillan, 1907.

[34] This letter from the *Gentleman's Magazine* is reprinted, *infra*, above footnote 53.

[35] Van Doren (ref. 25, *supra*), 168.

[36] This letter is reprinted, *infra*, above footnote 55.

[37] *Cf.* footnote 28, *supra*, and the discussion of Romas in § 8, *infra*.

pointed Rods to prevent the terrible Effects of Thunder-storms, I say, after all this, is not some Allowance to be made, if thy Crest is a little elevated? . . . I think, now I have stuck a Feather in thy Cap, I may be allowed to conclude in wishing thee long to wear it.

Thine,
P. Collinson.

I have not been able to locate the original of this letter, but it plainly could not have been written more than a day or two before 28 May, the date when Mazéas's letter was read at the Royal Society, and more likely it was written after 28 May. The portion printed above was quoted (without its date) by Franklin in a letter to Jared Eliot dated 12 April 1753. This letter begins:

The *Tatler* tells us of a Girl, who was observed to grow suddenly proud, and none cou'd guess the Reason, till it came to be known she had got on a new Pair of Garters. Lest you should be puzzled to guess the Cause, when you observe any Thing of the kind in me, I think I will not hide my new Garters under my Petticoats, but take the Freedom to show them to you, in a Paragraph of our friend Collinson's Letter, viz.—But I ought to mortify, and not indulge, this Vanity; I will not transcribe the Paragraph, yet I cannot forbear.[38]

Surely, this does not read like an introduction to an extract from a letter received by Franklin eight or ten months earlier. Rather, it gives the impression that Franklin had received Collinson's letter not too long before. That Collinson's letter had been in Franklin's hands when he wrote the kite letter on 1 October seems, therefore, very unlikely, although we must keep in mind that we have no direct evidence on way or the other.

The earliest dated letter from Collinson to Franklin mentioning the French experiments was written on 7 July 1752, and begins as follows:

I had the pleasure of my Dear friend's Letter of 21 March last with a guinea Inclosed but as I have Cash on hand I returned It by Moses Bartram. The Electrical Experim[en]ts have some thing very surprising in them, as all [thy letters] have. These our Friend Cave Intends to add to thy book as a Supplem-[en]t and then the Errata may be added before they are printed. Wee Shall wait the Return of the Autumn or Spring Ships. It's likely our Friend Kinnersley may add some others, under thy Direction.

By the Publick papers thou will See how thou has Sett the French to work.[39]

The last sentence above must refer to the accounts of the French experiments that were published in the *Gentleman's Magazine* or *London Magazine*. In a letter of 27 September 1752, Collinson told Franklin briefly that "all Europe is in agitation on Verifying Electrical Experiments on points—all commend the Thought of the Inventor—more I dare not say least I offend Chalk [?] Ears."[40]

Franklin must have received some word of the Marly experiments from Dalibard himself, but I have been unable to find a copy of this communication. In the "avertissement" to the second edition of his translation of Franklin's book, Dalibard wrote:

As soon as the first edition of this translation was completed, I sent a copy of it to M. Franklin, which put me into direct correspondence with him. I made known to him in time, the success of my experiment on thunder, I sent him the memoir which I had given to the Royal Academy of Sciences on 13 May 1752, such as it is in the second volume of the present work; he had been charmed by it and sent me, with his reply, his first supplement, the experiments of which I similarly verified. The second did not reach me until a long time afterwards.[41]

The earliest surviving letter I have found from Franklin to Dalibard is dated 29 June 1755. A truncated version of this letter was published in the *Philosophical Transactions* and was printed in Franklin's book on electricity, and in the various editions of his works.[42] The full text of this letter reveals that Franklin and Dalibard were never good correspondents. It begins:

[38] Smyth (ref. 33, *supra*) **3**: 124.

Volume 47 of the *Philosophical Transactions* (for the years 1751 and 1752), containing the Abbé Mazéas's letter, the accounts of the sentry-box experiments in France and in England and Germany, and also the letter about the kite, was printed in 1753. On 20 July 1753, Collinson wrote to Franklin about the antics of the Abbé Nollet who had been conducting a campaign against Franklin and now "his base and juggling intention" had been exposed: "Now on reading this & the King of France's approbation you will see in the Transactions, if the Furror Schould Rise again what will allay It—This will lead you to understand my former Paragraph. . . ." From an unpublished letter in the Library of the American Philosophical Society.

[39] The original manuscript letter is in the Library of the American Philosophical Society. *Cf. Benjamin Franklin's experiments* (ref. 4, *supra*), 124.

[40] Unpublished letter in the Library of the American Philosophical Society.

[41] P. 23; *cf.* reference 14, *supra*. Freely translated from the French.

[42] Smyth (ref. 21, *supra*) **3**: 269–273.

For a long time I have owed you a reply to your last letter, dated 20 June 1754. I received it last January while I was in Boston in New England,[43] & since that time I have been so busy with my travels to different places & with public affairs that I am extremely in arrears with my correspondents.

I sent you last year a manuscript which contains some new experiments & some observations on thunder; I do not know whether you have received it, but it has since been printed in London, & I imagine that our good friend Mr. Collinson will have sent you a copy.

I thank you for the kindness you have had in sending me the four volumes of natural history of M. de Buffon, the maps, *&c.*

You desire my opinion of *Père Beccaria's Italian* book. . . .[44]

In the fourth and fifth English editions of Franklin's book, this was printed as an "Extract of a letter concerning electricity, from Mr. B. Franklin, to Mons. D'Alibard, at Paris, inclosed in a letter to Mr. Peter Collinson, F.R.S." [45] Collinson wrote Franklin on 20 July 1753 that " your Letter is forwarded to Mons. Dalibard by a safe Conveyance as soon as I received it & a Supplement with It—one I sent before to Mr. Buffon." [46]

But even though there is no way of determining the dates of the earliest Dalibard-Franklin correspondence, we may be certain that Franklin had not learned of the Marly experiments from him by mid-September 1752, and very likely even by 1 October when he wrote the kite letter to Collinson. For, on 14 September 1752, when Franklin wrote to Colden that he saw in the May 1752 issue of the *Gentleman's Magazine* that a French translation of his book had appeared, he expressed the "hope [that] our Friend Collinson will procure and send me a Copy of the Translation." [47] We do not know exactly when Franklin received a copy, but some time in the fall of 1753 he had received Nollet's volume of letters attacking his and had sent it to Colden. On 25 October 1753, he wrote Colden: "I send you herewith Nollet's book. M. Dalibard writes me, that he is just about to publish an answer to it, which, perhaps, may save me the trouble." [48] Colden's son David read Nollet's book and sent Franklin some comments on it which he included in the second supplement to his own book; in a letter to Colden of 1 January 1754,[49] Franklin acknowledged the receipt of David's remarks, which had been enclosed in a letter from Colden "of the 3d past." In this same letter Franklin mentioned his having received from London "the Supplemental Electrical Experim[ts]" and added that "Mr. Dalibard wrote me that he was preparing an Answer" to Nollet.

The conclusion at which we arrive is that we have no way of telling when Collinson first wrote to Franklin about the success of the Marly experiment of Dalibard and those of Delor and Buffon, although it does not seem likely that he had mentioned this topic prior to his letter of 7 July 1752.

[43] Franklin sent this letter of Dalibard's to James Bowdoin (see footnote 13, *supra*) in a letter dated 13 December 1753, ". . . soon after my return from New England, I received the enclosed from Monsieur Dalibard, wherein he tells me that he is preparing an answer, not only to the Abbé, but to some others that have wrote against my doctrine, which will be published the beginning of this winter." From Smyth (ref. 32, *supra*) **3**: 192.

In Sparks (ref. 7, *supra*) **6**: 193–194, there is an English translation of a letter from Dalibard to Franklin dated 31 March 1754, beginning: "I received on the 15th of January last, your obliging letter of October 28th." This letter is given under the incorrect date of 1752 by Jernegan (ref. 7, *supra*), 182, n. 5, and Houston, Edwin J., Franklin as a man of science and an inventor, *Jour. Franklin Inst.* **161**: 286, 1906.

[44] Freely translated from Dalibard's French version in the second edition of his translation (Paris, 1756) **2**: 307–319. The remainder of the letter is as in Smyth, save that two short paragraphs are missing. The first (page 312) states that Franklin "will be very glad to learn about the experiments of M. le Roy on positive and negative electricity, when you will be able to communicate them to me." The second (page 319) is the conclusion of the letter: "I feel that the natural history of M. de Buffon will give me much pleasure & will instruct me enormously (*infiniment*). Assure him, I beg of you, of my respects as well as M. de Fontserrière, both of whom have given me tokens of their regard in your last letter. I am *&c.* B. Franklin."

[45] *Cf. Benjamin Franklin's experiments* (ref. 4, *supra*), 307–310. Smyth does not indicate that this letter is merely an extract.

[46] Unpublished letter in the Library of the American Philosophical Society.

[47] Reference 33, *supra*. If Franklin had not even received word of the French translation from Dalibard by 14 September, I assume he would not have received news of the Marly experiment directly from him until some time later than mid-September 1752.

[48] Smyth (ref. 32, *supra*) **3**: 164.

[49] Smyth (ref. 32, *supra*) **3**: 105–106 (following Sparks) printed this letter under the date 1 January 1753, as it is also printed in *Cadwallader Colden papers* **5**: 358–359 (*Collection N. Y. Hist. Soc.*, 1920). In modern style the date should obviously be 1 January 1754, since Franklin did not send Nollet's book to Colden until 25 October 1753. Furthermore, the supplement to Franklin's book on electricity was not yet available in America on 1 January 1753.

Franklin wrote Colden on 14 September that he had just read that there was a French translation of his book; had he previously known from Dalibard about the Marly experiments, he would have certainly been told by Dalibard that he was the translator of his book. Hence, I believe that there is no reason whatever to doubt Franklin's statement, as reported by Priestley, that he had not known of the French experiments when he claimed to have flown his kite in June 1752.

5. THE DATE OF FRANKLIN'S FIRST LIGHTNING ROD

The version of Franklin's letter to Collinson describing the kite as printed in the *Philosophical Transactions* differs from that which appeared in his book on electricity and which has been used by all the editors of collected and selected writings of Franklin, e.g., Sparks, Bigelow, and Smyth.[50] Whereas the date given in the *Philosophical Transactions* (and confirmed in the copy of the letter reproduced here as fig. 2) is 1 October 1752, the date given in Franklin's book and used by his editors is 19 October 1752. The latter is the date of the issue of the *Pennsylvania Gazette* containing the letter. Even more important than the date is the fact, first noticed by Hellmann,[51] that the final paragraph, although printed in the *Philosophical Transactions,* was omitted from the version in Franklin's book; it is also absent from the version in the *Pennsylvania Gazette* and the various editions of his writings.[52] This paragraph states:

> I was pleased to hear of the success of my experiments in France, and that they begin to erect points upon their buildings. We had before placed them upon our Academy and state-house spires.

Jernegan assumed that Franklin was referring to a report given in a letter from Paris, written on 26 May 1752 and published in the *Gentleman's Magazine* for May 1752. This letter was printed immediately following a long extract from Franklin's book, dealing with his lightning hypothesis, which was described as: "*A new* Hypothesis *for explaining the Phenomena of* Thunder, Lightning, *and* Rain. *Being an Extract from* B. Franklin's *Experiments and Observations on Electricity. Printed for* E. Cave, *and lately translated into* French *at* Paris." Following the extract from Franklin's book, we find:

> *The above Hypothesis is in part confirmed by some Experiments lately made in* France, *as appears by the following Letter from* Paris, *dated May* 26 N. S.
>
> From several electrical experiments performed by our best naturalists, in pursuance of those by Mr. *Franklin* in *Philadelphia,* to find whether the tonitruous and electrical matter be not analogous, it appears, that to fix on the highest parts of buildings or ships sharp-pointed iron bars of ten or twelve feet, and gilt to prevent rust, with a wire hanging down on the outside to the ground, or about one of the ship's shrouds, is a preservative against thunder. . . . The Sieur *Dalibard* having placed, in a garden at *Marly,* an iron bar on an electrical [i.e., insulated] body at the height of forty feet, was informed that on the tenth of *May,* about 20 minutes after two, a tempest passing over that spot, the parish priest and other persons drew from the bar such sparks and agitations as are seen in the common electrical performances. On the 18th the Sieur *de Lor* having fixed a bar at the height of 99 feet, on a cake of rosin, two feet square and three inches thick, drew coruscations from it during half an hour betwixt four and five, whilst the cloud was over it; these scintillations were perfectly like those emitted by his gun-barrel, when the globe is rubbed only with the brush, the same fire, the same crackling; whilst the rain, mixed with a little hail, fell from the cloud without any lightening or thunder, tho' it appeared to be the progress of a

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[50] Also Goodman, Nathan, *A Benjamin Franklin reader,* 381–382, N. Y., Thomas Y. Crowell Co., 1945; Mott, Frank Luther, and Chester E. Jorgenson, *Benjamin Franklin: representative selections, with introduction, bibliography, and notes,* 223–224, N. Y., etc., American Book Company, 1936.

[51] Hellmann, G., Ueber Luftelektricität, *Neudrücke von Schriften und Kartenüber Meteorologie und Erdmagnetismus* **11**, Berlin, 1898.

This issue containing facsimiles of publications by Winckler, Franklin, Dalibard, and Lemonnier. Rotch (ref. 22, *supra*) called attention to Hellmann's discovery of the discrepancy between the two versions of this letter.

[52] The various editors of Franklin's writings based their versions on previous editions, hence ultimately on the first supplement to the first English edition of Franklin's book on electricity: *Supplemental experiments and observations on electricity, part II. Made at Philadelphia in America, by Benjamin Franklin, Esq.; and communicated in several letters to P. Collinson, Esq.; of London, F. R. S.,* London, E. Cave, 1753. [This is E1.2 in the classification given in *Benjamin Franklin's experiments* (ref. 4, *supra*), 141.] The final letter of that supplement, suppressed in the fourth and fifth English editions (1769 and 1774, respectively), printed immediately following the kite letter, was addressed to Collinson and read: "As you tell me our friend *Cave* is about to add some later experiments to my pamphlet, with the *Errata,* I send a copy of a letter from Dr. *Colden,* which may help fill a few pages; also my kite experiment in the *Pensylvania Gazette*; to which I have nothing new to add, except the following experiment towards discovering more of the qualities of the electric fluid. . . ." *Cf. Benjamin Franklin's experiments,* 161, note 29.

A Cut and Deſcription of a Machine, eaſily conſtructed, for making the Experiment by which Franklin's *new Theory of Thunder is demonſtrated.*

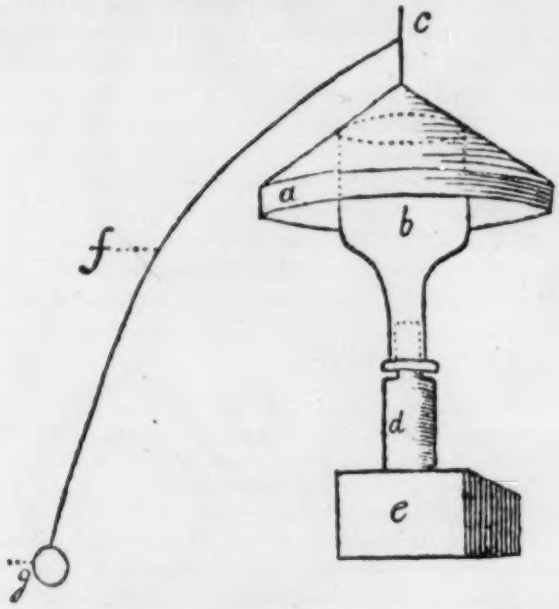

FIG. 4. Among the extensive discussions of Franklin's theory that lightning is an electrical phenomenon, and the various experiments demonstrating electrification clouds, that appeared in the *Gentleman's Magazine* for 1752, was a letter describing the simple apparatus shown here, contrived so that any one might perform the experiment for himself.

"*a*, Is a tin veſſel, ſuch as is uſed to cover diſhes. *b* is a quart bottle or decanter. Pour a ſufficient quantity of cement into the tin cover, and then preſs the bottom of the bottle upon it, that when the cement is cold they may adhere firmly together. On the top of the tin cover let there be fixed the ſpike *c* of any metal, and about 18 inches in length, terminating in a ſharp point: then having ſtuck up the ſhort ſtick *d*, in the block *e*; put the neck of the bottle over the other end, firſt cutting it with a ſhoulder, to prevent its going in too far. Place this machine on the top of the houſe in the open air, and having faſtened the wire *f* to the ſpike *c*, bring it down to the window of the room in which the effect is to be obſerved, and faſten to the end of it the ball *g*, which may be of any metal, and need not be more than a quarter of an inch in diameter. When a thick cloud paſſes over the place, the ſpike will become electrified, and the electricity being prevented from running off by the glaſs bottle, will deſcend along the wire, and the ball being touched will emit the electrical ſparks. A perſon ſtanding on wax, if in contact with the wire, will become electrified, and may kindle ſpirits with his finger. In like manner on Mr *Franklin's* principles, if a ſharp ſpike be placed on the top of a maſt with a wire conductor faſten'd from it to the ſhrouds, it will prevent the maſt from ſplitting, and the ſhip from damage by lightning. *Yours &c.*

"*N. B. The wire muſt not come in contact with any ſubſtance that is a conductor of electricity.*" [*Gentleman's Mag.* **22**: 327, 1752.]

In the "hiſtorical chronicle" of the same magazine for August 1752, there appeared a short account of the experiments in England made by Canton and Wilson "in conſequence of the experiments lately made in *France* purſuant to Mr. *Franklin's* doctrine of thunder and lightning, and his ſuggeſtion of drawing off gradually the electrical fire from clouds by pointed bars." The account ended: "On this occaſion we recommend to all gentlemen who take the laudable pains of keeping meteorological journals, that they would be diligent in watching what ſort of clouds do chiefly affect the machine, and ſend their obſervations to be communicated to the public. It is easy to annex two little bells to the wire of the machine, with a clapper between, which without further trouble will give notice when it is electrified. Mr. *Watkins*, at Sir *Iſaac Newton's Head, Charing Croſs*, keeps them to ſell ſo prepared." [*Ibid.* **22**: 383, 1752.]

tempest which had happened elsewhere. Both these experiments have been reported to the Royal Academy of Sciences, and both evince that thunder clouds may be deprived of their fire, by iron bars fashioned and fixed as above.[53]

We know that Franklin had read this account before writing to Collinson on 1 October since, as we have already seen, he referred to this issue of the *Gentleman's Magazine* in a letter to Colden dated 14 September 1752. We may note that the anonymous author of this Paris letter states that the experiments had been devised to discover whether "the tonitruous and electrical matter be not analagous," and that the conclusion warranted by the demonstration of the "analogy" was that sharply pointed iron bars—affixed to the highest parts of buildings and ships and grounded—are a "preservative against thunder."

I have found evidence that Franklin knew about such experiments even earlier than 14 September 1752. The *London Magazine*, during this period, was reporting the news about electricity just as the *Gentleman's Magazine*. The issue of the *London Magazine* for May 1752 had an almost identical letter from Paris, describing the work of Dalibard and Delor, which Franklin had before him when writing to Collinson on 14 September. The *London Magazine* for June 1752 carried a supplementary note on the French lightning experiments which reads as follows:

[53] *Gentleman's Mag.* **22**: 229, May 1752. *Cf. supra*, note 34.

To what was said of Lightning and Electricity in our last, . . . we shall add the following, which is also from Paris, June 12.

Tho' many very able and experienced naturalists have many years ago asserted, that lightning and the power of electricity were one and the same thing; which notion was grounded on the resemblance there was between their respective phenomena; yet resting satisfied with the conjecture only, they never pointed out any ways or means for the demonstration of the fact. Mr. Francklyn, however, of Philadelphia in America, carried this critical point much further, and has pointed out the means for making the experiment; in which particular point he has succeeded beyond expectation. Mr. Lemonier,[54] in particular, one of his most Christian majesty's physicians in ordinary, who is a member of the Academy Royal of Sciences, made the experiment accordingly at St. Germain en Laye, during the tempest which happened on the 7th instant; and planted in the garden of the Hotel de Noailles, an iron rod for that purpose. He plainly perceived, that at the first flash of lightning that fell on it, the rod was electrified in the same manner, and had visibly the same appearance, as it would have had in case it had been electrified according to art. Abundance of persons of indisputable credit were eye-witnesses of the effects it produced; from whence it is now demonstrable, that the effects of lightening and electricity are the same.

The July 1752 issue contained three communications on lightning as follows:

Further Remarks and Experiments in relation to Lightning and Electricity. . . .

Paris, June 30. Upon the steeple of the church of Plauzat, in Auvergne, is a cross of iron, not painted or gilt. The extremities of this cross form sorts of fleurs-de-lis with sharp points. Whenever there happens any great storm, accompanied with thick clouds and flashes of lightening, a luminous body is perceived upon every one of the extremities of this cross. According to an immemorial tradition, there very rarely happens to be any thunder at Plauzat, or in the neighbourhood, when this phenomenon appears. As soon as it is seen, people are certain that the storm is no more to be feared. The luminous bodies are of different colours like the rainbow, and the figure is conical. Sometimes they continue an hour and an half, if it rains ever so plentifully.

Brussels, July 3. The Sieur Torre having caused a pointed iron rod to be erected upon the top of his house, on the 23d ult. at night, tho' there was but a slight appearance of a storm, shining sparks were drawn from that rod; on the 26th at night a dark cloud covered the sky, and a heavy rain, mixed with hail, fell when people were surprized to feel and to see, that a finger held at the distance of two inches from the rod, excited very strong sparks. These phaenomena greatly increased upon a clap of thunder being heard.

Paris, July 7. M. le Noine,[54] the king's physician, has made a new experiment in electricity, at St. Germaine en Laye, which confirms the analogy of the effect it has to that of thunder; the weather being very cloudy, he caused a cake of rosin to be brought to the place, upon which he mounted, and without any other instrument he extended his hand above his head, as a thicker cloud than ordinary passed over him, and one of those who were with him having touched him to make him remark something, he instantly received a most violent shock, of which fact he has made report to the Royal Academy of Sciences.

The same issue contained, in the section entitled "Foreign Affairs," further information on Dalibard's experiments:

M. Dalibard, who frequently exhibits electrical experiments, got a bar of iron, or rather several joined together, to the length of 50 or 60 feet, erected at a village 7 or 8 miles from Paris, on the road to Compeigne: it was suspended by silken cords, and rested on glass bottles; so that supposing it could be electrified, it would not part with its virtue. One day a cloud passed over and discharged a clap of thunder, at which time M. Dalibard could draw sparks of fire from the bar, even at the distance of several inches. The flashes and sparks produced the pricking sensations as those from the conductor in the usual experiments. The diverging lucid stream was seen to issue from the pointed end of the bar; and every thing concurred to prove indisputably, that the bar was strongly electrified by the cloud. A gentleman who assisted at the experiment, upon slightly touching the rod unawares, received a violent stroke on his arm, and his clothes smelt all over of sulphur. The whole academy was entirely satisfied with the account, which clearly proved, that the matter of thunder and electricity is one and the same thing; and that it was practicable to extract thunder from a cloud, and direct it which way we please.

Franklin knew of these reports in the *London Magazine*. In the issue of the *Pennsylvania Gazette* for 27 August 1752, he published an "Extract of a Letter from Paris," taken from the May issue of the *London Magazine*, of which the text is substantially the same as that in the May issue

[54] Lemonnier or Le Monnier made a series of experiments, some in concert with Mazéas, with insulated rods and found that the experiment worked equally well if the rod was horizontal rather than vertical. His most significant results dealt with the electrification of the rod in the absence of thunder-clouds, a topic brought to a high state of investigation by the Abbé Beccaria (for whom see, *infra*, § 8). For some account of Lemonnier's research, see, *supra*, references 19, 51.

of the *Gentleman's Magazine* which I have reprinted above. On 28 September, three days before he wrote to Collinson about the lightning kite, he published another account of lightning experiments in the *Pennsylvania Gazette,* this one being similar in content to the note printed above (from the July issue of the *London Magazine*). The text printed by Franklin is even closer to (though not a word-for-word reprint of) a note in the "historical chronicle" of the *Gentleman's Magazine* for July 1752. Franklin's note reads:

> Brussels, July 3. The Sieur Torre having caused a pointed Iron Rod to be erected upon the Top of his House, with Design, in some Measure, to dissipate the Fire which is in the Air, during the Time of a Storm, has succeeded therein beyond his Expectation; On the 23d of last Month, at Night, though there was but a slight Appearance of a Storm, shining Sparks were drawn from that Rod, but weaker than those drawn from an electrify'd Bar; on the 26th at Night a dark Cloud covered the Sky, and a heavy Rain, mix'd with Hail, fell, when People were surprised to feel and to see, that a Finger held at the Distance of two Inches from the Rod, excited very strong Sparks. These Phaenomena greatly increased upon a Clap of Thunder being heard; insomuch that the Sparks grew considerably longer, larger and brighter. There is Reason to believe by this Experiment, that the pointed Rod or Bar may be of great Use in diminishing the Quantity of Fire from whence Thunder is formed, and in preventing the fatal Effects of Lightning.[55]

Hence, when Franklin wrote to Collinson on 1 October 1752 about how glad he was to learn that the "French" were beginning to erect "points" on their buildings, he could hardly have helped having in mind the item from Brussels that he had just published in the *Gazette* five days earlier. This note states that Monsieur Torre's rod had been erected early enough to enable him to perform lightning experiments on 23 June 1752.

Even more relevant is a note that Franklin later printed in the *Pennsylvania Gazette* (9 November 1752), which reads:

> Paris, August 5. Several Persons of Quality, have ordered Iron Rods to be fixed on the Tops of their Houses, to preserve them from the Thunder.

As we shall see in the next section, it is not entirely clear that Torre's rod was designed *primarily* for protective purposes, whereas those referred to in the above note dated 5 August 1752 were to be erected solely for the purpose of preserving houses "from the Thunder." I cannot help feeling that this news item was in Franklin's hands when he wrote the letter about the kite to Collinson on 1 October 1752. Franklin wrote about his pleasure in hearing of the success of his experiments in *France*—and I presume that he knew that Brussels was not in France—"and that they there begin to erect points on their buildings," a plain reference to more than one lightning rod erected on a building in France. Two months would have been sufficient time for news to have reached Philadelphia from Europe. Hence, I believe that the concluding sentence of Franklin's letter about the kite may legitimately be interpreted to mean that he had erected rods on two public buildings in Philadelphia earlier than the beginning of August 1752, i.e., in June or July 1752.

This conclusion is consistent with the report by Priestley that the kite experiment was performed in June 1752; since I assume that the lightning rods would not have been erected prior to experimental verification of the electrical nature of lightning, and there seems no indication of any other verification as early as June 1752 save the kite experiment.

Franklin's earliest suggestion of a lightning rod was included in a communication entitled "Opinions and conjectures, concerning . . . electrical matter, arising from experiments and observations, made at Philadelphia, 1749," and enclosed in a letter written by Franklin to Collinson dated 29 July 1750.[56] This is the same communication in which the sentry-box experiment was proposed. After describing a series of experiments in which charged insulated conductors had been discharged by a near-by pointed conductor, he wrote:

> . . . may not the knowledge of this power of points be of use to mankind, in preserving houses, churches, ships, &c. from the stroke of lightning, by directing us to fix on the highest parts of those edifices, upright rods of iron made sharp as a needle, and gilt to prevent rusting, and from the foot of those rods a wire down the outside of the building into the ground, or down round one of the shrouds of a ship, and down her side till it reaches the water? Would not these pointed rods probably draw the electrical fire silently out of a cloud before it came nigh enough to strike, and thereby secure us from that most sudden and terrible mischief?[57]
>
> To determine the question, whether the clouds that contain lightning are electrified or not, I would propose an experiment. . . .

[55] *Cf. supra,* note 36.

[56] Reference 12, *supra.*

[57] For further information on Franklin's early descriptions of lightning rods, see § 6, *infra.*

Then followed the proposal to erect a sentry box on the top of a high tower or steeple.

It should be noted that the only question in Franklin's mind that demanded experimental proof was whether "the clouds that contain lightning are electrified." Once proofs were at hand that such is the case, then there would be no doubt whatever in his mind that lightning rods would work and would deprive the clouds of their electrical fire before they could discharge it in a bolt of lightning. Sufficient laboratory data had been accumulated to support his view; if the clouds were electrified and if, then, the lightning discharge was merely a bigger spark discharge than that obtained in the laboratory (not in any way different in kind), there was certainly no reason to suppose that the change in scale would affect the action of pointed conductors in discharging charged bodies—whether small metal laboratory objects or gigantic clouds.

To do the protective job assigned to them, lightning rods had merely to be fixed on "the highest parts" of "houses, churches, ships, &c." By contrast, the test rod in the sentry-box experiment was thought by Franklin to require "some high tower or steeple." In all likelihood, the greater elevation envisioned for the test rod was to ensure a large effect, since Franklin knew that a pointed conductor will "draw off" the charge from a charged insulated conductor with greater "ease" at near distances than from afar, and he obviously would have wanted the results of the sentry-box experiment to be on a sufficiently large scale to be convincing. According to Priestley, Franklin "was waiting for the erection of a spire in Philadelphia to carry his views into execution; not imagining that a pointed rod, of a moderate height, could answer the purpose." It has been assumed that the spire in question was that on Christ Church.

If we accept Priestley's word that Franklin flew his kite in June, and that this experiment successfully indicated the electrification of thunderclouds, then Franklin had no further reason to delay the introduction of lightning rods to protect buildings in Philadelphia.

6. WHAT KIND OF LIGHTNING ROD DID FRANKLIN ERECT IN PHILADELPHIA IN 1752?

Many points of confusion exist with regard to early lightning rods. First, the distinction between grounded and ungrounded rods is not always made clear. Second, the action of the rods to prevent a stroke is not always kept distinct from their action in successfully conducting a stroke into the ground. Third, a considerable ambiguity exists about how proof may be obtained that the lightning rod is a "preservative against thunder."

In Franklin's communication of 1750, embodying the sentry-box experiment,[56] he described two types of lightning rod. One was grounded and its purpose was to "draw the electrical fire silently out of a cloud before it came nigh enough to strike, and thereby secure us from that most sudden and terrible mischief." We may note that such *protective* rods, as recommended by Franklin, were always to be grounded. The second type of lightning rod, to be used in the sentry-box experiment, was ungrounded or insulated. The reason why this *test,* or *experimental,* rod was not grounded is, of course, that its function was to become charged when a cloud passed overhead. In terms of Franklin's theory, a grounded rod would draw off the electrical fluid from clouds and transmit this fluid into the ground, an indefinite reservoir, until the charge on the cloud (its excess electrical fluid) was all removed; in a very short time, the cloud would be discharged and an experimental test would be difficult. Actually, as we shall see below, a grounded rod will become charged by induction when an electrified cloud is overhead, and such charge will be bound there so long as the cloud is above the rod; hence *we* know that the experiment can succeed as well with a grounded rod as an insulated one. Franklin's idea was to use an insulated rod, which would draw off some but not all of the electrical fluid of the cloud, which would remain on the rod somewhat longer since it would not be immediately dissipated into the ground, but only by the slower point discharge whereby electrified bodies "throw off" their excess electrical fluid if they are pointed.

Franklin was aware of possible hazard to a man standing near an ungrounded rod during a storm, as in the proposed sentry-box experiment, even though he stood on an insulating stand. He suggested, therefore, that the man might draw sparks from the charged insulated rod by means of a grounded wire with an insulating wax handle, so that "the sparks, if the rod is electrified, will strike from the rod to the wire, and not affect him." Even so, he declared his faith that, as to possible danger, "I think there would be none," and he did not take similar protective steps in performing the kite experiment. Richmann's death a few years later, while performing a variation of Franklin's

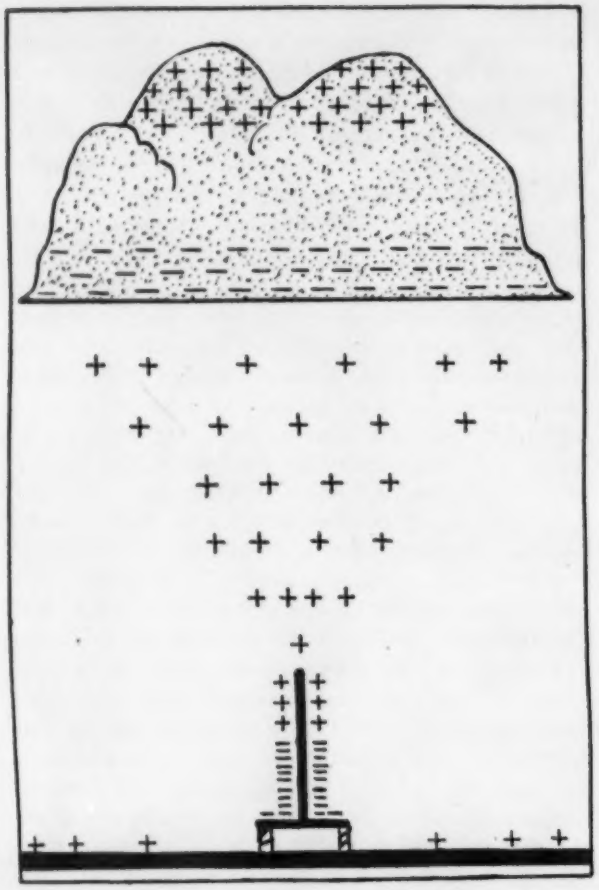

FIG. 5. Effect of an electrified cloud on an insulated rod. Reproduced, by permission, from Schonland, B. F. J., *The flight of thunderbolts*, 18, Oxford, Clarendon Press, 1950.

sentry-box experiment, indicated that the danger in such experiments was greater than Franklin had envisaged; although we may note that when Richmann was electrocuted during a lightning storm, he was standing on the floor and not on an "electrical" (or insulated) stand.

The lightning experiments performed in France, Belgium, Germany, and England in 1752 were made with insulated rods or test rods, which had the function (by design) of indicating the electrification of overhead clouds, not of affording protection. As I pointed out earlier, Franklin was certain that a grounded pointed rod would afford protection from the lightning—if, that is, the thunder-clouds were to prove to be electrically charged. Just as an insulated pointed conductor would draw off *some* of the electrical fluid from charged clouds, so a grounded pointed conductor would draw off *all* the electrical fluid from such clouds and render them harmless, i.e., incapable of occasioning an electrical discharge, or lightning stroke. We have seen this idea expressed in his communication of 1750 (describing the sentry-box experiment). It also was stated in another letter of the same year (probably a little earlier), in which Franklin suggested that the end of the rod might, for greater effectiveness, be "divided into a number of points": ". . . the electrical fire would, I think, be drawn out of a cloud silently, before it could come near enough to strike."[58] In "Poor Richard's Almanack" for 1753, announced in the *Pennsylvania Gazette* for 19 October 1752, Franklin merely indicated that the lightning would be attracted by the grounded rod and so preserve houses and ships from damage.[59]

The process whereby an insulated iron rod becomes charged is, according to our present state of knowledge, somewhat different from what Franklin envisioned. He assumed that if a cloud, say positively charged or with an excess of electrical fluid, passed directly overhead, then the insulated rod would draw off some of the electrical fluid from the cloud and become itself positively charged. We hold that the rod, in the presence of an overhead positively charged cloud, will exhibit positive electrification at the lower end and negative electrification at its upper end in a process known as "electrostatic induction"; incidentally, the first clear notions of electrostatic induction are due to Franklin, even though he did not apply them to this case.[60] If the electric potential is sufficiently great, some of the charge on the upper end of the rod will leak off, forming a glow discharge (which Franklin likened correctly to St. Elmo's fire or the "sailors' *corpusantes*"), and will move upward, being carried by ions (charged air molecules), and neutralize some of the charge on the bottom of the

[58] This letter was published in the *Gentleman's Magazine* **20**: 208, May 1750. *Cf.* Jernegan (ref. 7, *supra*), 189, note 26.

[59] The description of the lightning rod in Poor Richard for 1753 has been often reprinted. We may note that it advocated grounded, pointed, metallic conductors for buildings and ships: "A House thus furnished will not be damaged by Lightning, it being attracted by the Points, and passing thro the Metal into the Ground [or water, in the case of ships] without hurting any thing." The complete issue of Poor Richard for 1753 is reproduced in facsimile in Mott and Jorgenson (ref. 50, *supra*), 225-260.

[60] Franklin first applied the concept of electrostatic induction to the charging of a Leyden jar. This point is discussed at length in the writer's monograph (see reference 1, *supra*).

cloud. In discussing this phenomenon, B. F. J. Schonland, one of the foremost investigators of the lightning discharge in our time, notes: "The process goes on until the rod has acquired a considerable excess of charge *of the same sign as that on the base of the cloud*. Franklin's experiment does not draw electricity from the cloud but has the same effect as if it had done so." [61] Hence, we see how a positively charged cloud (or, a cloud whose lower portion is positively charged) causes an insulated rod to discharge negative electricity from its upper end, so as to be left positively charged and appear as if it had drawn off some electrical fluid from the positively charged cloud. In the same way, if a negatively charged cloud (or, a cloud whose lower portion is negatively charged) passes over the rod, the upper end of the rod becomes positively charged, the ions streaming upwards are positively charged, so that the rod is left with a residual negative charge, just as if the cloud had drawn electrical fluid from the rod. Hence the sign of the charge on an insulated rod provides (as Franklin believed) a reliable index to the sign of the charge on clouds passing overhead, or at least the sign of the charge on the lower part of the cloud. This process is indicated in figure 5.[62]

In the case of the rod being grounded, we have the situation indicated in figure 6. The "repelled negative 'induced' charge is no longer on the rod (having been repelled to a great distance) and the rod will discharge positive electricity so long as the cloud is near enough."[63] But, we must ask, does this phenomenon occur on a sufficiently large scale for the rod to be able to disarm the clouds of their charge and prevent a stroke? Schonland remarks on this score:

> As stated originally by Franklin, it [i.e., the lightning rod] depended for its success or failure upon the degree to which the upward discharge of electricity between pointed rod and cloud could render harmless the charge on the cloud. The lightning-rod, if it was to work in this manner, could only do so if the point discharge from it did actually neutralize the charge on the cloud to an appreciable extent. In the laboratory experiments which Franklin had made with his electrical machine, earth-connected metal points certainly neutralized electrified bodies placed near to them. But it would seem unlikely that the same thing could happen on the much larger scale of Nature when a puny point, a few tens of feet high, faced a thunder-cloud a mile or more above it.
>
> In actual fact it does not happen. A single point, or for that matter a multitude of points such as the tops of trees in a forest or the poles and chimneys of a town, has little effect upon the charge on the thunderstorm above it. None the less the lightning-rod, as is abundantly proven, has a very real virtue, because it "attracts" lightning to it and can lead a flash to ground without damage to the building to which it is attached.[64]

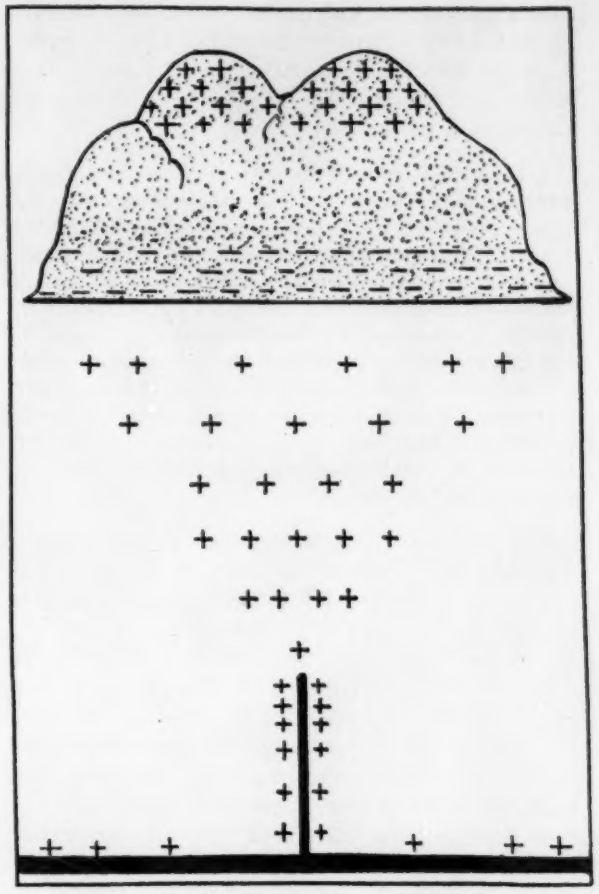

FIG. 6. Effect of an electrified cloud on a grounded or non-insulated rod. Reproduced, by permission, from Schonland, B. F. J., *The flight of thunderbolts*, 19, Oxford, Clarendon Press, 1950.

Franklin was quick to learn that a lightning rod can "also" protect a ship or a building by attracting the lightning and safely conducting the charge into the ground. Many of those who objected to the lightning rods did not appreciate

[61] Schonland, B. F. J., *The flight of thunderbolts*, 17, Oxford, Clarendon Press, 1950.

[62] The previous paragraph and the following one are based on the lucid discussion given by Schonland.

[63] Schonland (ref. 61, *supra*), 17.

[64] *Ibid.*, 23.

that Franklin advocated these two modes of action of rods: (1) to prevent a stroke, (2) to conduct a stroke into the ground.[65] On 29 June 1755 he wrote to Dalibard that he had been "but partly understood in that matter":

> I have mentioned it in several of my letters, and except once, always in the *alternative, viz.* that pointed rods erected on buildings, and communicating with the moist earth, would either *prevent* a stroke, *or,* if not prevented, would *conduct* it, so as that the building should suffer no damage. Yet whenever my opinion is examined in *Europe,* nothing is considered but the probability of those rods *preventing* a stroke of explosion, which is only a *part* of the use I proposed for them; and the other part, their conducting a stroke, which they may happen not to prevent, seems to be totally forgotten, though of equal importance and advantage.[66]

We must now ask what kind of lightning rod Franklin had reference to when he wrote to Collinson on 1 October 1752 that "points" had been erected on buildings in Philadelphia in June 1752. Were they ungrounded test rods as devised for the sentry-box experiment or were they grounded rods for protective purposes? In his letter to Collinson, Franklin wrote: "I was pleased to hear of the success of my experiments in France, *and* that they begin to erect points upon their buildings." I have italicized the word *and,* since it emphasizes the sense of Franklin's letter as I read it, which seems to indicate that Franklin was pleased to learn that points were being erected upon buildings in addition to his pleasure at the success of his experiments. The points referred to by Franklin as having been earlier erected in Philadelphia were, I believe, designed to protect the Academy[67] and the State House from lightning. Franklin, I am sure, would not have exposed these two public buildings to any possibility of danger and he knew by the summer of 1752 that an ungrounded rod entailed some hazard; at the least, it did not provide the best possible protection. In his communication of 1750 to Collinson, and again in Poor Richard in the autumn of 1752,[68] he insisted in the plainest terms possible that protective lightning rods be grounded.

Although Jernegan[69] believed that the reference to "points" on the Academy and State House implied that they were not for protection, but were rather test rods, he did not raise the question of whether they were grounded or insulated. He argued that the appearance of the sentence about "points" erected in Philadelphia in a letter "concerned with the identification of lightning and electricity" implies that the "natural interpretation of these words would be that he placed 'points' on the 'Academy and state-house spires' for the purpose of experiment, and not for protection." But we have no indication of any experiment ever performed with these "points." Nor did Franklin ever imply elsewhere that he had used an insulated rod for lightning experiments before hearing of such experiments from Europe. Priestley insisted that Franklin had flown the kite before receiving the news of the European experiments, but neither he nor Franklin ever stated that he had performed the experiment with the insulated rod. Jernegan cited Priestley's statement:

> The Doctor, after having published his method of verifying his hypothesis concerning the sameness of electricity with the matter lightning was waiting for the erection of a spire in Philadelphia to carry his views into execution, not imagining that a pointed rod of a moderate height could answer the same purpose; when it occurred to him that by means of a common kite he could have readier and better access to the regions of thunder than by any spire whatever.

Jernegan concluded: "This would indicate that in June, the date given for the kite experiment, that 'points' had not been erected on high buildings at that date even for experimental purposes." But, as Franklin was to learn from Europe, the experiment does not require that rods be placed as high as he had thought. If he had actually erected experimental rods, he would have performed experiments on them and he never claimed to have done so. Furthermore, he would not then have needed to erect yet another experimental rod, as he did on his house in September. Finally, I cannot see why he would have erected two experimental rods—one would have been ample. I would, therefore, agree that no experimental rods had been erected in Philadelphia in June 1752, but conclude that those on the Academy and State House must, therefore, have been protective or grounded rods.

[65] *Cf.* the writer's article, Prejudices against the introduction of lightning rods, *Jour. Franklin Inst.* (in press).

[66] *Benjamin Franklin's experiments* (ref. 4, *supra*), 307–308.

[67] The fore-runner of the present University of Pennsylvania.

[68] Although the issue of Poor Richard containing the account of the lightning rod was "1753," the text of it must have been prepared earlier than 19 October 1752, since the *Gazette* for the latter date carried an advertisement that "Poor Richard's Almanack" was "on the press."

[69] Reference 7, *supra.*

FIG. 7. Portrait of Benjamin Franklin showing the lightning rod he erected in his house in September 1752. This portrait was painted by Mason Chamberlin in 1762 when Franklin was fifty-six years of age and is reproduced from an engraving made after the portrait by Freeman, and, according to the legend, was "Sold by M. Chamberlin in Stewart Street, Old Artillery Ground, Spittalfields." The price was five shillings. It shows the two bells with which the portions of the lightning rod terminated, the little ball [or clapper] suspended by a thread between the bells, and the two balls suspended by strings from the bell on the right, forming an electroscope, and indicating the presence of an electrified cloud above.

In September 1752, Franklin erected a lightning rod on his own house in Philadelphia. It took an interesting form and it provided the means for making an important discovery. This rod was contrived "to draw the lightning down into my house, in order to make some experiments on it, with two bells to give notice when the rod should be electrify'd: A contrivance obvious to every electrician," as Franklin wrote to Collinson in September 1753.[70] This instrument[71] was described by Franklin in greater detail in his paper "Experiments, observations, and facts, tending to support the opinion of the utility of long, pointed rods, for securing buildings from damage by strokes of lightning, read at the committee appointed to consider the erecting of conductors to secure the magazines at Purfleet, August 27th, 1772" as follows:

In Philadelphia I had such a rod fixed to the top of my chimney, and extending about nine feet above it. From the foot of this rod, a wire (the thickness of a goosequill) came through a covered glass tube in the roof, and down through the well of the staircase; the lower end connected with the iron spear of a pump. On the staircase opposite to my chamber door, the wire was divided; the ends separated about six inches, a little bell on each end; and between the bells a little brass ball, suspended by a silk thread, to play between and strike the bells when clouds passed with electricity in them.[72] [See fig. 7.]

Franklin's description of this rod as a device "to draw the lightening down into my house, in order to make some experiments on it" has, I believe, misled the modern historian. Jernegan observed, "It is clear, however, that this was not a rod set up for protection but for experimental purposes." Yet Franklin's description of the action of the rod indicates that, in addition to its use in experiment, it could provide adequate protection. Franklin related:

After having frequently drawn sparks and charged bottles from the bell of the upper wire, I was one night awaked by loud cracks on the staircase. Starting up and opening the door, I perceived that the brass ball, instead of vibrating as usual between the bells, was repelled and kept at a distance from both; while the fire passed, sometimes in very large, quick cracks from bell to bell, and sometimes in a continued, dense, white stream, seemingly as large as my finger, whereby the whole staircase was inlightened as with sunshine, so that one might see to pick up a pin.[73]

Since the separation of the bells was less than six inches, any quantity of lightning sufficient to do damage to an unprotected house could arc across this air gap and so be carried from the upper member of the rod to the lower member and be successfully conducted into the ground without causing any damage. Franklin knew that his rod would afford protection to his house on the basis of his investigations of the path followed by lightning when buildings were struck.[74] Thus, in his letter to Collinson describing the lightning rod with the bells, he wrote:

[70] Smyth (ref. 33, *supra*) **3**: 149. The discovery made by Franklin was that clouds appear to be charged negatively more often than positively and that, therefore, "for most part, in thunder-strokes, *'tis the earth that strikes into the clouds, and not the clouds that strike into the earth.*"

[71] A similar device was described by Dalibard in the second French edition of his translation of Franklin's book on electricity, **1**: lxxviii (histoire abregée) and **2**: 130–132, as having been invented by him a few days after he had made public his account of the Marly experiment (12 May 1752) and placed into operation on the rod which he had erected for Buffon at the *jardin du Roi*. The pointed insulated rod ended in a bell, like Franklin's, but the second bell was not attached to a grounded rod but was merely attached "à la muraille."

[72] Smyth (ref. 33, *supra*) **5**: 421.

[73] *Idem.*

[74] In 1758, while Franklin was in London, he wrote a letter to his wife about the bells: "If the ringing of the Bells frightens you, tie a Piece of Wire from one Bell to the other, and that will conduct the lightning without ringing or snapping, but silently," Smyth (ref. 33, *supra*) **3**: 441. This letter plainly indicates that Franklin knew that the lightning was conducted through his rod system into the ground by a series of spark discharges as well as the "convection" set up by the clapper, a device similar to that introduced by Franklin to discharge the two coatings of a Leyden jar and to prove that the charges on the two coatings were equal in absolute magnitude though opposite in sign.

The subsequent history of this rod system is described in Montgomery, Thomas Harrison, *A history of the University of Pennsylvania from its foundation to A. D. 1770*, 75, note 18, Phila., George W. Jacobs Co., 1900: "These earlier experiments of Franklin were carried on in the house built by John Wister, No. 141 (now 325) Market street in 1731. 'It was in this house that Dr. Franklin . . . erected his first [?] lightning rod, an hexagonal iron rod, still in our possession, connecting it with a bell which gave the alarm whenever the atmosphere was surcharged with electric fluid. The ringing of the bell so annoyed my grandmother that it was removed at her request.' *Memoir of Charles J. Wister*, by his son, 1866, vol. i, pp. 21, 23. John Wister's son, Daniel, who was born 4 February, 1738–39, was a pupil at the Academy 1752-1754, as was also his cousin Caspar in 1752."

It would be interesting to know whether the family papers of the Wisters, or of other Academy pupils in 1752, contain any references to "points" or to the kite experiment.

In every stroke of lightning, I am of opinion that the stream of the electric fluid, moving to restore the equilibrium between the cloud and the earth, does always previously find its passage, and mark out, as I may say, its own course, taking in its way all the conductors it can find, such as metals, damp walls, moist wood, &c. and will go considerably out a direct course, for the sake of the assistance of good conductors; and that, in this course, it is actually moving, though silently and imperceptibly, before the explosion, in and among the conductors; which explosion happens only when the the conductors cannot discharge it as fast as they receive it, by reason of their being incomplete, dis-united, too small, or not of the best materials for conducting. Metalline rods, therefore, of sufficient thickness, and extending from the highest part of an edifice to the ground, being of the best materials and complete conductors, will, I think, secure the building from damage, either by restoring the equilibrium so fast as to prevent a stroke, or by conducting it in the substance of the rod as far as the rod goes, so that there shall be no explosion but what is above its point, between that and the clouds.[70]

One of the best known instances of Franklin's tracing the path of lightning in order to show how the lightning will depart from a simple path "to pass as far as it can in metal" occurred when the Newbury (Mass.) church was struck and was described in a letter Franklin wrote to Dalibard on 29 June 1755.[66] But Franklin was aware of this phenomenon at least as early as August 1752, since he published in the *Pennsylvania Gazette* for 6 August the following account:

Last Friday, early in the Morning, the Lightning struck two Houses on Society Hill, and did them considerable Damage, but hurt no Person. It was very remarkable in both Houses, that the Lightning in its Passage from the Roof to the Ground, seem'd to go considerably out of a direct Course, for the sake of passing thro' Metal; such as Hinges, Sash Weights, Iron Rods, the Pendulum of a Clock, *&c.* and that where it had sufficient Metal to conduct it nothing was damag'd; but where it passed thro' Plaistering or Wood work, it rent and split them surprizingly.[75]

I believe that the lightning rods erected in Philadelphia in 1752 were the first grounded lightning rods to be erected anywhere in the world for the purpose of protecting buildings from the lightning discharge. Dalibard's rod at Marly-la-Ville was erected in the middle of a garden and was insulated. Delor's rod was erected on his house in Paris, but it too was insulated, as was the apparatus used by LeMonnier, Mazéas, LeRoy, Cassini de Thury, and the Abbé Nollet. In England, Canton erected an insulated instrument built for the occasion of a tin tube with needles attached to the top and Wilson performed the experiment with an iron curtain rod projecting out of the window. Mylius and Ludolf in Berlin likewise used insulated rods.

The letters in the *Gentleman's Magazine* and the *London Magazine* often referred to the fact that experiments with insulated rods had proved the efficacy of lightning rods, but such statements do not imply that the insulated test rods were afterwards grounded in order to become effective protective rods, nor that rods for protection had been erected.[76] After quoting the letter from the *Gentleman's Magazine* for May 1752, *viz.*

From several electrical experiments performed by our best naturalists, in pursuance of those by Mr. *Franklin* in *Philadelphia*, to find whether the tonitruous and electrical matter be not analogous, it appears, that to fix on the highest parts of buildings or ships sharp-pointed iron bars of ten or twelve feet, and gilt to prevent rust, with a wire hanging down on the outside to the ground, or about one of the ship's shrouds, is a preservative against thunder.

Jernegan concluded that Franklin, who had read this letter, "knew by September 14, 1752 that there was a report that French scientists had placed iron rods on buildings and that they were 'a preservative against thunder.'" But the "report" does not warrant this conclusion. Plainly, it seems to me, the report implies that since experiments (with ungrounded conductors) have proved that lightning clouds are electrified, or that the "tonitruous and electrical matter" are "analagous," then (as Franklin has pointed out, since electrified clouds must follow the same laws as electrified bodies in the

[75] I believe that this important item, not hitherto cited in discussions of early lightning rods, confirms my earlier statement that Franklin would not have erected a wholly ungrounded lightning rod on a public building in Philadelphia in the summer of 1752. The implication is plain that lightning, attracted by the rod, would follow the metal as far as possible, but would then have to complete its path by travelling through plaster or wood work which it would rend and split. The rod erected by Franklin on his house in September 1752 avoided this danger since the choice of a path through metal meant that it had only to cross a small air gap and would not thereby hurt the house in any way. Hence, if Franklin had erected lightning rods in Philadelphia in the early summer of 1752, as I believe he did, they must have been grounded.

[76] A close reading of the reports in the *Phil. Trans.* (see ref. 19, *supra*) and those in the *Gentleman's Magazine* and *London Magazine* (quoted in § 5, *supra*) indicate that in every case the argument reads that (1) since lightning is electrical, (2) rods of some sort [grounding is not specified] can protect buildings or ships.

laboratory) "it appears" that a pointed grounded conductor "is a preservative against thunder."

An understandable confusion arises, of course, from the very nature of the experiments with insulated rods. They appeared to draw some "tonitrous matter" from the clouds and, thereby, become electrified. But, in drawing off "tonitruous matter," did they not lessen the striking power of the cloud overhead and so offer some protection? In this light, we can understand the beginning of the report from Brussels, which Franklin published in the *Pennsylvania Gazette,* in which Monsieur Torre's rod was said to have been erected "with Design, in some Measure, to dissipate the Fire which is in the Air," but the remainder of the communication does not make it clear as to whether the rod was grounded or insulated. Yet this account is written in terms so similar to current reports on insulated rods, such as those printed above, that I cannot help feeling that his rod, too, may have been insulted.

The Paris report of 5 August 1752 stated merely that several persons of quality had ordered protective rods to be erected on their houses, and if Franklin's specifications were used, these rods would have been grounded. I have been unable to find when (or, for that matter, whether) these rods were actually erected.

The "divided" lightning rod with bells that Franklin erected on his house in September 1752 afforded adequate protection and, if my reading of the final sentence in the kite letter be correct, two public buildings in Philadelphia were protected by rods in June or July 1752.

Eight years later, when Franklin was in England, he wrote Kinnersley that, despite the importance of the lightning rod "on our side of the water," . . . "Here it is very little regarded; so little, that though it is now seven or eight years since it was made publick, I have not heard of a single house as yet attempted to be secured by it." [77] Two months earlier, on 24 January 1762, he had sent the philosopher David Hume a description of the method for constructing lightning rods, omitting "the philosophical reasons and experiments on which this practice is founded; for they are many, and would make a book. Besides they are already known to most of the learned throughout Europe." Although the "philosophical reasons and experiments" were known throughout Europe, the practice was evidently not. "In the American British colonies," continued Franklin, "many houses have been, since the year 1752, guarded by these principles." [78]

[77] Smyth (ref. 33, *supra*) **4**: 146.

7. FRANKLIN'S DELAY IN REPORTING THE KITE EXPERIMENT (AND OTHER SUBJECTS OF DOUBT IN THIS AFFAIR)

If, as I have attempted to show, there is no reason to doubt that Franklin had conceived and executed the kite experiment before hearing the news of the French performance of the sentry-box experiment, the reader may well inquire why so much space is necessary for the investigation of this question. Why should we not simply accept Franklin's and Priestley's word: especially since Franklin's statement in his letter to Collinson of 1 October 1752 and Priestley's account form a consistent picture with all of the evidence cited above? The answer lies in the fact that the June date has been seriously questioned and some investigators have concluded that Franklin must have flown the kite much later, at a time when he had already heard of the successful conclusion of the sentry-box experiment in France.[79] Furthermore, the statement has been made more than once that Franklin did not perform the experiment at all, or that, if he did, he did not report his results accurately in the letter to Collinson, which is then supposed to be a plan of an experiment to be made rather than an account of one that has already been made. Furthermore, as I pointed out in the beginning of this article, a statement has been made in the pages of the *Proceedings* of this Society to the effect that the experiment is nothing but a myth.

The most ardent critic of the kite experiment was Alexander McAdie, who concluded his article on "The date of Franklin's kite experiment" [23] with the following statement:

> The whole tenor of the letter of October 1 (19) 1752, indicates not so much an experiment actually performed as one projected and the results anticipated. For actually the phenonema are quite different. Franklin does not say in the concluding paragraph

[78] *Ibid.*, 129.

[79] In other words, I have accepted Priestley's date of June 1752 and have indicated that since Franklin heard of the French experiments later in the summer, we have evidence consistent with Priestley's other statement that Franklin flew his kite before he learned what had been done in Europe. Others, e.g., Rotch (see ref. 22, *supra*), for whatever reasons, have assumed that Franklin must have performed the kite experiment after hearing of the French experiment and have then shown that he could not then have flown the kite as early as June.

that he actually charged a phial, etc. Only that it may be charged.[80]

McAdie gave a particular drubbing to Dr. Stuber, pointing out that his "account is in general terms; and what is rather surprising, explanatory and apologetic." Stuber's account is as follows:

> It was not until the summer of 1752, that he was enabled to complete his grand and unparalleled discovery by experiment. The plan which he had originally proposed, was, to erect on some high tower, or other elevated place, a sentry-box, from which should rise a pointed iron rod, insulated by being fixed in a cake of resin. Electrified clouds passing over this, would, he conceived, impart to it a portion of their electricity, which would be rendered evident to the senses by sparks being emitted, when a key, the knuckle, or other conductor was presented to it. Philadelphia at this time afforded no opportunity of trying an experiment of this kind. While Franklin was waiting for the erection of a spire, it occurred to him that he might have more ready access to the region of clouds by means of a common kite. He prepared one by fastening two cross sticks to a silk handkerchief, which would not suffer so much from the rain as paper. To the upright stick was affixed an iron point. The string was, as usual, of hemp, except the lower end which was silk. Where the hempen string terminated, a key was fastened. With this apparatus, on the appearance of a thunder-gust approaching, he went out into the commons, accompanied by his son, to whom alone he communicated his intentions, well knowing the ridicule which, too generally for the interest of science, awaits unsuccessful experiments in philosophy. He placed himself under a shade, to avoid the rain—his kite was raised—a thunder-cloud passed over it—no sign of electricity appeared. He almost despaired of success, when, suddenly, he observed the loose fibres of his string to move towards an erect position. He now presented his knuckle to the key, and received a strong spark. How exquisite must his sensations have been at this moment. On this experiment depended the fate of his theory. If he succeeded, his name would rank high among those who had improved science; if he failed, he must inevitably be subjected to the derision of mankind, or, what is worse, their pity, as a well-meaning man, but a weak, silly projector. The anxiety with which he looked for the result of his experiment, may be easily conceived. Doubts and despair had begun to prevail, when the fact was ascertained in so clear a manner that even the most incredulous could no longer withold their assent. Repeated sparks were drawn from the key, a phial was charged, a shock given, and all the experiments made which are usually performed with electricity.[81]

I must agree with McAdie's acerb comment that if by the statement, "he placed himself under a shade," Stuber implied that Franklin stood under a tree to escape rain, he certainly was unaware of Franklin's "own previously published warning that it was very dangerous to stand under trees during a thunder-storm."[82] On the other hand, I am not so sure that I agree with McAdie's stricture: "Why should one who had made an estimate of what we may call the killing power of lightning, wish to expose his own son to probable death or at any rate intense shock?" I am sure that Franklin did not believe that he was taking his own life and that of his son into his hands when he performed this experiment. We may note, in this regard, that in his description of the original experiment of the sentry-box, he had stated that he did not believe there was any danger of the experimenter being killed by the lightning discharge during the performance of the experiment. McAdie points out that Franklin would hardly have gone to the common if he had desired to fly his kite "where none could see and comment." But he was thinking of a New England common in the center of the town, whereas the Philadelphia common was on the outskirts.

McAdie directed his criticism at Stuber, we may note, rather than at Priestley. "Dr Stuber knew Franklin intimately and it is said got the story of the kite from him," he declared. How reliable a witness was Stuber? An early nineteenth-century edition of Franklin's works referred to him as "one of the Doctor's [i.e., Franklin's] intimate friends."[83] Sparks stated cautiously that Stuber, "who resided in Philadelphia, . . . seems to have

[80] This is the final part of McAdie's conclusion, of which the remainder has been quoted, *supra*, above footnote 29.

[81] Quoted by McAdie. The above version has been corrected by a comparison with *The complete works in philosophy, politics, and morals, of the late Dr. Benjamin Franklin* **1**: 107–109, London, J. Johnson & Longmans, Hurst, Rees, Orme, and Brown, second edition [no date].

[82] *Cf.* Franklin's letter to Dr. John Mitchel of 29 April 1749 "containing observations and suppositions, towards forming a new hypothesis for explaining the several phaenomena of thunder-gusts," in *Benjamin Franklin's experiments* (ref. 4, *supra*), 209, in which after noting that "As electrified clouds pass over a country, high hills and high trees, lofty towers, spires, masts of ships, chimneys, *&c.* as so many prominencies and points, draw the electrical fire, and the whole cloud discharges there," he concluded, "Dangerous, therefore, is it to take shelter under a tree, during a thunder-gust. It has been fatal to many, both men and beasts."

[83] Reference 81, *supra*, 98.

written from minute and accurate information." [84] Parton stated, "We owe our knowledge of what occurred on the memorable afternoon [on which the kite was flown], to two persons who had heard Franklin tell the story, namely, Dr. Stuber of Philadelphia and the English Dr. Priestley." [85] (What warrant there may be for placing the experiment in the afternoon, I do not know.) George Simpson Eddy, who provided McAdie with much of his information concerning Stuber, stated: "I have an edition of the Life and Essays of Dr. Franklin published in the Republic of Letters, a journal which was published in New York in . . . 1834. This life begins in No. 2 of that journal, page 171. On page 180 begins the continuation of Franklin's life written by Stuber, who is described as 'one of the Doctor's intimate friends.'" [86] Eddy apparently was willing to accept this statement that Stuber was a close personal friend of Franklin and did not express any disapproval of Parton's statement that Franklin had told the story in person to Dr. Stuber.

I do not know what the evidence is that Dr. Stuber was an intimate friend of Franklin, nor even that he got the story at first hand from Franklin.[87] There is no correspondence extant between Stuber and Franklin; he is not mentioned in any Franklin letter in the Smyth, Bigelow, or Sparks editions of Franklin's writings, nor was he of sufficient importance to be mentioned in Carl Van Doren's biography. The only information contained in Stuber's account, which is not to be found in Priestley's, is open to serious question, as McAdie pointed out, and leads one to believe that Stuber merely added a few personal embroideries to the account which he had obtained from Priestley.

But even if we may not place too much confidence in Stuber, I see no reason to doubt the credibility of Franklin's and Priestley's testimony. McAdie could not believe that Franklin had flown his lightning kite in June because no account of the experiment appeared in the newspapers: it seemed to him "quite improbable that a man so astute as Franklin and so keenly aware of the importance of this particular experiment, would have failed to publish a note, however brief, and preliminary, in the *Gazette*." Yet the evidence indicates that Franklin was not in the habit of publishing brief and preliminary notes about his scientific discoveries in the *Gazette;* in fact, the publication of the kite letter in the issue of October 19 is, so far as I have been able to tell by an examination of the files of the *Gazette* during the years in which Franklin made his experiments, a solitary exception. And even in this case we may note that a communication had gone off to Collinson (and through him to the Royal Society) more than two weeks before the letter was published in the *Gazette*.

McAdie's next doubt was as follows:

> What is perhaps still more significant, E. Kinnersley, who was the chief expositor of the newly-discovered electric fire, and who was in close correspondence with Franklin (Franklin borrowed his "brimstone globe" March 2, 1752, and used it in making experiments in the spring of 1752) gave several public lectures, in which there is no mention of the kite experiment. In the Pennsylvania Gazette of September 14, 1752, there is an account of Kinnersley's lecture at the State House. And again in the issue of September 21, September 28, and October 19.[88]

[84] Sparks (ref. 7, *supra*) **5**: 173.

[85] Parton (ref. 7, *supra*) **1**: 295.

[86] From a letter written by Eddy to McAdie, 15 December 1923, quoted by McAdie (ref. 23, *supra*), 16–17. Eddy wrote: "Parton, Vol. 1, page 289, says (referring to the spring of 1752), 'nearly three years have rolled away since he had suggested *in his private diary* a mode of ascertaining whether lightning and electricity were really the same.' I do not know what Parton meant by 'private diary.' I think he must have been referring to the paper written by Franklin in 1749 and entitled 'Opinions and Conjectures, concerning the Properties and Effects of the Electrical Matter, arising from Experiments and Observations, made at Philadelphia, 1749.'"

Despite a number of errors, Parton usually knew what he was talking about and referred to genuine items. The "private diary" in question was one from which Franklin took an extract which he included in a letter to Dr. Lining dated 18 March 1755, which read: "*Nov.* 7, 1749. Electrical fluid agrees with lightning in these particulars: 1. Giving light. 2. Colour of the light. 3. Crooked direction. 4. Swift motion. 5. Being conducted by metals. 6. Crack or noise in exploding. 7. Subsisting in water or ice. 8. Rending bodies it passes through. 9. Destroying animals. 10. Melting metals. 11. Firing inflammable substances. 12. Sulphureous smell.—The electric fluid is attracted by points.—We do not know whether this property is in lightning.—But since they agree in all the particulars wherein we can already compare them, is it not probable they agree likewise in this? Let the experiment be made." *Cf. Benjamin Franklin's experiments* (ref. 4, *supra*), 334.

[87] I have been unable to locate any biographical information concerning Stuber, save an occasional mention of his name.

[88] For Kinnersley's career, see *Benjamin Franklin's experiments* (ref. 4, *supra*), appendix one.

What reason can there be for Franklin having given Kinnersley the opportunity of making the first announcement of the lightning kite in Philadelphia? Considering the importance of the experiment, would we not rather have expected Franklin to have reserved for himself the first public statement about the kite, as he apparently did?

Finally, McAdie noted, "It would also seem that, once assured of the results, Franklin would have wasted no time in communicating with Peter Collinson to have the paper laid before the Royal Society." There are, I believe, two plausible reasons why Franklin might have delayed his report from June to October.

First, we must remember that Franklin was often slow (especially by current standards) in sending reports of his experiments to England. Thus the paper entitled "Further experiments and observations in electricity" was enclosed in a letter to Peter Collinson dated Philadelphia 29 April 1749, beginning: "Sir, I now send you some further experiments and observations in electricity made in Philadelphia 1748. viz . . ."; the famous paper entitled "Opinions and conjectures, concerning the properties and effects of the electrical matter, arising from experiments and observations, made at Philadelphia, 1749," was not sent to Collinson until 29 July 1750; and the letter written to Collinson in September 1753 (about the negative electrification of clouds) noted the dilatoriness at the very beginning when Franklin wrote, "In my former paper on this subject, written first in 1747, enlarged and sent to England in 1749, ..." The letter last mentioned, written in September 1753, described experiments which Franklin performed during the period from September 1752 to 6 June 1753.

A second possible reason for the delay, it seems to me, is that Franklin may very well have hoped for another opportunity to repeat the experiment before writing a full report for Collinson and the Royal Society. We know from a letter which Franklin wrote to John Perkins on 13 August 1752 that his affairs were pressing, that "business sometimes obliges one to postpone philosophical amusements."[89] The kite experiment was an almost incredible performance, and Franklin knew it. He might, therefore, well have deemed it necessary to have performed this experiment at least once more before publishing a formal account of it. This would certainly explain why Franklin did not write an account of the kite to Collinson immediately after hearing of the news about the French experiments, which, as I have shown above, occurred toward the end of August.

Yet another reason for the delay has been advanced by Van Doren in his splendid biography of Franklin. According to Van Doren, might not Franklin "deliberately have kept his secret till October so that he might publish at the same time, or almost the same time, in his newspaper and in his almanack the two most important pieces of his year's news? That is what he did. On 19 October his first account of the *Electrical Kite* appeared in the *Gazette*. The same issue advertised as in the press the new *Poor Richard* for 1753, which contained Franklin's first positive statement of *How to Secure Houses, etc., from Lightning*."[90] The chief weakness in this argument, however, is that a description of lightning rods had already been published in the first edition of Franklin's book on electricity, which had already been out for a year.

Very likely, the delay on Franklin's part was caused by a multitude of factors, and the most important may have been a fear of ridicule. Many a modern commentator has called this experiment "foolhardy." Others still find it remarkable that Franklin and his son were not electrocuted. It certainly would have taken a great deal of courage for anyone to have said he had actually drawn down the lightning from the sky, or even that he had dared to fly a kite during a thunder-storm. Priestley points out, in the report which had its origins in Franklin's statements to him, that Franklin, "dreading the ridicule which too commonly attends unsuccessful experiments in science, . . . communicated his intended experiment to nobody but his son, who assisted him in raising the kite." To be sure this is a reference to the possibility of ridicule in case of failure, yet the notion of ridicule was clearly present in Franklin's own mind—even when he told Priestley about the experiment years later.

There can be no question but what it was difficult to take seriously a proposal to test the electrification of clouds by drawing down the lightning from heaven. The June issue of the *Gentleman's Magazine* (1752) contained "a letter from a gentleman at Paris to his friend at Toulon, concerning a very extraordinary experiment in electricity, dated May 14, 1752," which began:

[89] Smyth (ref. 33, *supra*) **3**: 95–97.

[90] Van Doren (ref. 25, *supra*), 165.

You must remember, Sir, how much we ridiculed Mr *Franklin's* project for emptying clouds of their thunder, and that we could scarce conceive him to be any other than an imaginary Being. This now proves us to be but poor *virtuosi;* for yesterday I met a learned gentleman of the academy, who assured me that the experiment had been very lately tried with success. You may suppose I could scarce think him serious; however, I found that a memoir read at one of their assemblies had made so extraordinary an impression upon him, that I began myself to abate of my incredulity.

This gentleman was probably not alone in his sentiments, and I am sure that there were many who not only ridiculed Franklin's sentry-box experiment when it was first proposed, but who also could scarcely believe it when they were told that such an experiment had been successful. The British members of the Royal Society of London, despite the approval by some of them of Franklin's lightning hypothesis, did not make the experiment that Franklin had proposed. We do not know exactly why, and the reason may well have been that the "connoisseurs" laughed at it.[91] The laughter is all the more remarkable when we consider the splendid reception his earlier papers had received, even his preliminary statements on the electrification of clouds.[92] One use made of Franklin's conception of the electrification of clouds, as Collinson had written to him in April 1750, was by those who wished to solve the phenomena of earthquakes.[93]

The news that man had been able to draw the lightning from the skies was certainly astounding. Could the reports be true? We must remember that the first news of the lightning experiment originated in France; how many agreed with Collinson's sentiments in a letter to Franklin of 20 July 1753: "Wee know the French Very Well, subject to Levity, Hights & Extreams"?[94] When Watson published in the *Philosophical Transactions* of the Royal Society an account of the experiments with the insulated rod made by John Canton, Benjamin Wilson, and John Bevis, he noted that the effects were "trifling" when "compared with those which we have received from Paris and Berlin, but they are the only ones, that the last summer here has produced." Nevertheless, "as they were made by persons worthy of credit, they tend to establish the authenticity of those transmitted from our correspondents."[95] Surely the last part of this sentence indicates that there was not a universal trust placed by Englishmen in the reports from France and Germany.

It seems to me, therefore, that an important reason why Franklin did not at once make public the results of the lightning experiment in June was the fear that no one would take him seriously; he did not want to compromise his reputation. After he had heard of the news from France, he was then willing to publish a brief account of the kite, since he now had independent confirmation of what he had proved by means of this experiment.

No one who reads the pages of the *Pennsylvania Gazette* for the period from June to October 1752 can help but notice the many references to lightning, chiefly accounts of storms and their destructive effects during the early summer and news of European experiments in the late summer and early fall. Quite obviously, the subject of lightning was on Franklin's mind, as it well might have been if he had already performed

[91] "One Paper which I wrote for Mr. Kinnersley, on the Sameness of Lightning with Electricity, I sent to Dr. Mitchel, an Acquaintance of mine, and one of the Members also of that Society [i.e., the Royal Society of London]; who wrote me word that it had been read but was laught at by the Connoisseurs." Farrand (ref. 15, *supra*), 382.

[92] For example, Collinson wrote Franklin on 5 February 1750, "Your very Curious Pieces relating to Electricity and Thunder-Gusts have been read before the Society & have been Deservedly admired not only for the Clear Intelligent Stile, but also for the Novelty of the Subjects." *Benjamin Franklin's experiments* (ref. 4, *supra*), 80; *cf.* also pp. 82–84.

[93] Chiefly the Rev. William Stukeley, F.R.S., who mentioned Franklin by name in a paper explaining how earthquakes and lightning have the same cause—the electric fluid. *Cf. Benjamin Franklin's experiments* (ref. 4, *supra*) and the article cited in reference 65, *supra.*

Stukeley entered the following comment in his diary, after hearing Franklin's letter of 1 October read at the Royal Society: "21 December 1752. At the Royal Society. Mr. Franklin, of Philadelphia, sent a pretty account of his extracting fire from the clouds, as singular in the invention as less operose and costly than those of the French astronomers. He makes a cross of two bits of cedar wood, tyes a silk handkerchief to the points by its corners, sets up a small iron half a foot long on that point which is the head of the kite, applys tail and wings to it as usual, a bunch of ribbands is to be between the end of the string and your hand, and then you fly it as ordinary kites when a cloud passes by loaden with the electric fire, and then you thus draw it down.—*Diary*, vol. xii., 2." From: The family memoirs of the Rev. William Stukeley, M.D. (2), *Publications of the Surtees Soc.* **80**: 466–467, 1885. *Cf.*, also, the article referred to in reference 65, *supra.*

[94] *Benjamin Franklin's experiments* (ref. 4, *supra*), 114, written apropos of the antics of Nollet in attempting to prove that Franklin's experiments would not work.

[95] *Phil. Trans. Roy. Soc.* **47**: 569, 1751 and 1752.

the kite experiment and had erected two lightning rods in Philadelphia. One such account, originating from Portsmouth, New Hampshire, 23 July 1752, and published in the *Gazette* for 6 August, was followed by a post-script of Franklin's reading: "A plain Proof of the Electrick Nature of Lightning." This post-script is all the more interesting in that the notice preceding it did not actually indicate such a proof at all, and simply stated:

> The main Mast of a Schooner at the North-end was struck by the Lightning; and altho' the Mast was shiver'd to Pieces by it (and the other Mast ruined by the Shock) till it came to a Ring that encompassed it (which it melted a little) yet below that Ring there were no Effects of it—A plain Proof of the Electrick Nature of Lightning.

Does this not read as if Franklin, having proved "the Electrick Nature of Lightning" (by the kite experiment), could not help adding a conclusion which, if not warranted by the facts reported from New Hampshire, was uppermost in his mind? He certainly knew what a "proof" was.

If we accept Priestley's statement that the kite experiment was performed in June, then Franklin's letter to Collinson of 1 October 1752 indicates that he did not hesitate long before erecting rods on at least two buildings in Philadelphia.

The kite experiment had proved to his own satisfaction that thunder clouds are electrified, or are charged bodies; hence according to everything that he had learned about electricity, the lightning rods should work. On this basis, we can understand why he did not include, in the version of his letter on the kite which he published in the *Pennsylvania Gazette,* the final paragraph about the erection of "points"; I believe that this topic is fully understandable as explained by Van Doren: "The rods on the Academy and State House were already known to Philadelphia." [96]

It is well to re-emphasize that once Franklin knew that clouds were electrified, he was certain that the lightning rods must work as he had predicted, since there was no reason to suppose that electrified clouds would behave differently with regard to pointed grounded conductors than ordinary electrified bodies in his laboratory. Jernegan and others indicate that Franklin had not in 1752 "proved by experiment that they [lightning rods] were a 'preservative against thunder.'" The only real proof to be had of the efficacy of lightning rods in preserving houses against lightning required that a bolt of lightning hit the rod and not destroy the house. The first such occasion arose eight years after the lightning rods had been first erected, i.e., in 1760, when the house of Mr. West was struck and was saved from destruction by the lightning rod which had been erected on it.[97]

If we can understand why Franklin might have held back any public announcement of the kite experiment from June until October, we must still answer certain other objections raised by McAdie. One of them is that Franklin did not repeat the kite experiment. Once Franklin had heard of the French experiments, which provided all the information he desired, further experiments with a kite were unnecessary. In September 1752, as we saw previously, he erected a form of lightning rod in his own house which provided him with ample means of making experiments on electrified clouds. The statement in his autobiography makes clear that Franklin thought that his sentry-box experiment as performed by Dalibard was superior to the kite experiment; certainly it was easier to perform. Once Franklin had heard, late in August 1752, of the European experiments, he immediately (September 1752) constructed the dual-purpose rod (to serve as a protective instrument for his house and as an experimental tool), since he now knew that the test instrument did not have to be as high as he had originally supposed. With this instrument, he easily verified for himself the results obtained in Europe and those previously obtained by him with the kite. Having, now, more than once proved the electrification of thunder-clouds, he wrote a note to Collinson about the kite.[98] All of Franklin's subsequent research on lightning was done with the rod, although (as we shall see in the next section) Kinnersley made further experiments with the kite.

[96] Van Doren (ref. 25, *supra*), 169.

[97] Sparks (ref. 7, *supra*) **5**: 375–377.

[98] Two further objections of McAdie to Priestley's account and Franklin's may be mentioned briefly. One is that the style ["tenor"] of the letter "indicates not so much an experiment actually performed as one projected and the results anticipated." I suppose McAdie referred to the form of statements such as, "You will find it [the electric fire] stream out plentifully . . .," or "the phial may be charged, . . . spirits may be kindled, . . .," etc., rather than "I found it . . .," or "a phial was charged, . . . spirits were kindled," etc. But a similar style was used by Franklin in earlier communications, e.g., that to Collinson of 1 September 1747, in *Benjamin Franklin's experiments* (ref. 4, *supra*), 179 ff.: Expt. III, "If a cork suspended by a silk thread hang between these two wires, it will play incessantly from one to the other . . ."; Expt. IV, "Place an electrised phial on wax . . ."; Expt.

8. THE LIGHTNING KITE EXPERIMENT IN THE MID-EIGHTEENTH CENTURY, AND ROMAS'S CLAIM TO PRIORITY

Among those who performed experiments with lightning kites in the mid-eighteenth century, one of the most interesting is the Frenchman Jacques de Romas, *assesseur au présidial de Nérac*,[99] since he claimed priority in the invention of this instrument. In a book supposedly dealing with lightning conductors, and the means of protecting houses from thunder, he devoted a considerable amount of space to establishing the grounds for his independent discovery. He related that "the first experiment on the electricity of thunder [at Marly] was announced to the public by all the gazettes and other periodical works,"[100] and he then decided to repeat these experiments, not—as he says—because he doubted their veracity, but rather to see whether there were new phenomena to be explored, which might be important "for the utility of civil society, or the progress of physics." He made some experiments with an insulated rod or bar but, wishing to increase the effects, he "plunged himself into meditation." Finally after one half hour the idea of the kite (*le cerf-volant des enfants*) presented itself to his mind. In a letter which he wrote to the Académie de Bordeaux on 12 July 1752 he announced his plan to use as a means of exploring the electrification of clouds "un Jeu d'enfant."[101] However August passed and the time of thunder-storms was over. He therefore waited until the following winter had passed and did not raise his kite until 14 May 1753, while a second experimenter watched the insulated rod erected on his house so that the two types of observation might be coordinated.

Romas insisted that "un Jeu d'enfant" referred unambiguously to a kite, and I see no reason to doubt his word. The paramount question in Romas's mind was whether Franklin had actually performed his kite experiment in June 1752, as Priestley said he had in his history of electricity, or whether Franklin had done it later. Plainly, to establish his own priority, Romas had first to show that Priestley's attribution of the month of June must be an error. But even assuming that Franklin's experiment had been made at the end of June, Romas wanted to prove that it would have been impossible for him to have received news of it earlier than the thirteenth of July; in other words, even if Franklin had thought of the kite earlier than he had, at least he wanted credit for independent invention. Romas asked: if Franklin had known him personally and had sent him a special message about the kite experiment at the end of June, could it have arrived in Bordeaux in

VI, "Place a man on a cake of wax, and present him the wire of the electrified phial to touch, you standing on the floor, and holding it in your hand. As often as he touches it, he will be electrified *plus;* and any one standing on the floor may draw a spark from him." At the end of his "Farther experiments and observations in electricity, 1748," Franklin referred to some experiments he and his friends planned to perform, and the style is quite marked: "Spirits, at the same time *are to be* fired. . . . A turkey *is to be* killed . . ." [my italics].

McAdie also states, "Franklin's conception, or perhaps the interpretation put upon the experiment and generally accepted, was that a cloud was a reservoir of electricity and the kite string a conductor. On the contrary, it appears to have been purely induction, not conduction. Had the kite string been wet enough to act as a conductor, the fibres would not have stood out." Two objections are indicated. First, the cord available in Franklin's day was a better conductor than McAdie supposed. For example, Stephen Gray's famous experiments on conduction and insulation and on induction made use of "packthread" for the conductor and silk lines for the insulator. But such cord is a better conductor when damp or wet, so that the system of metal in the kite, cord, and key was a better conductor when the rain had dampened the cord. Even if the phenomenon were, as pointed out earlier, one of induction and not conduction, the somewhat conductive (slightly) damp cord would produce the effect of fibres sticking out at a certain stage. McAdie may have underestimated the conductive power of eighteenth-century cord [packthread].

[99] *Cf.* Abria, Jérémie J. B., Rapport sur l'éloge de Romas, *Actes de l'Académie des Sciences, Belles-Lettres et Arts de Bordeaux* **15**: 441–446, 1853.

The Academy has also published a volume containing [1] Table historique et méthodique (1712–1875), [2] Documents historiques (1711–1713), and [3] Catalogue des Manuscrits de l'ancienne Académie (1712–1793), Bordeaux, Imprimerie G. Gounouilhou, 1879; *cf.* p. 39 (No. 150), p. 61 (No. 349, No. 350), p. 62 (No. 351, No. 353, No. 355), for descriptions of his unpublished communications, and for some account of MSS, see p. 185, p. 244 (No. 119), p. 245 (No. 142), p. 249 (No. 300, No. 315, No. 316, No. 317), p. 250 (No. 328 through No. 341), p. 278 (No. 1056), p. 279 (No. 1064)—the MSS in this list deal with scientific subjects, chiefly meteorology and electricity; they were (in 1877) in the Bibliothèque de la Ville (Bordeaux). We may note that two of the above MSS dealt with the trisection of the angle and perpetual motion. *Cf.* also p. 331 (No. 1552), p. 333 (No. 1556).

Three publications by Romas are listed by me in footnotes 100 and 109, *infra*, and a long study of his work in footnote 106, *infra*.

[100] *Mémoire sur les moyens de se garantir de la foudre dans les maisons*, 7, Bordeaux, Bergeret, 1776.

[101] *Ibid.*, 12. This letter is printed on pp. 105–106; it was apparently read at a meeting of the Academy on 17 July.

FIG. 8. One of the many experiments made by Romas with his electrical kite. Frontispiece to Romas, Jacques de, *Mémoire sur les moyens de se garantir de la foudre dans les maisons; suivi d'une lettre sur l'invention du cerf-volant électrique, avec les pièces justificatives de cette même lettre,* Bordeaux, Chez Bergeret, 1776. (Reproduced from A. L. Rotch's copy, now in Houghton Library, Harvard University.)

as little as thirteen days? But, he had never heard tell of Benjamin Franklin in June 1752 "and I do not have enough vanity to flatter myself that at this same time I had the honor to be known to him," [102]—to say nothing of the possibility of a ship getting the message from Philadelphia to Bordeaux within thirteen days! The first news of the alleged kite experiment of Franklin ("la prétendue expérience du Cerf-volant de M. Franklin" [103]), according to Romas, only arrived in the hands of his London correspondents by January 1753 [104] and it did not arrive in France until the fifteenth of January when Watson wrote a letter to Nollet about it; how then "could I have been informed about it by 12 July 1752?"

Romas had heard of the Marly experiment by reading an account in the *Gazette de France* for 27 May 1752, a copy of which arrived at Nérac, where he was stationed, only in the first days of June. [105] He assumed that at least an additional month would have been required to get the news to Philadelphia. Hence, if—as he believed—Franklin flew his kite only after hearing of the experiments of Dalibard and Delor, he could not have flown his kite in June as Priestley had asserted. Romas claimed, furthermore, that Priestley himself had indicated that Franklin's kite experiment postdated his learning of the French sentry-box experiments. The quotation which follows was alleged by Romas to have been taken from the 1767 (first English) edition of Priestley's history, and it was repeated *verbatim* by Merget in a supposedly definitive article on Romas written for the Academy of Bordeaux in the nineteenth century.

M. Franklin est le premier (nous savons maintenant à quoi nous en tenir sur cette priorité), qui ait soupçonné l'identité des éclairs et du fluide électrique; il a indiqué d'avance le moyen de constater cette identité, en proposant d'isoler à l'air libre, en temp d'orage, une aiguille électrisable par communication; le premier spectacle électrique que cet instrument ait offert, a paru en France sous les yeux de MM. de Lor et d'Alibard. *M. Franklin, animé par le succès de ces deux Messieurs,* éprouva lui-même le succès de son aiguille à Philadelphie, où il était alors. Ce physicien ayant eu aussi un heureux succès, pensa bientôt qu'au moyen d'un cerf-volant il pourrait se procurer un accès plus sûr et plus facile dans la région où s'engendre la foudre: l'idée de ce moyen se trouva juste, par l'épreuve qu'il en fit au mois de juin de la même année 1752, dans la compagne de Philadelphie, où il jugea à propos d'opérer sans autre témoin que son fils, pour éviter la risée des sots.[106]

It will be noted that the italicized phrase exactly contradicts the sense of Priestley's own words. This text as a whole does not come from Priestley's history at all,[107] although several phrases (e.g., the end of the final sentence) do; I suspect that Romas

[102] *Ibid.*, 109, 132–133.

[103] *Ibid.*, 110.

[104] *Ibid.*, 110; Romas was mistaken, since Franklin's letter about the kite was read at a meeting of the Royal Society on 21 December 1753.

[105] *Ibid.*, 133.

[106] Merget, [?], Etude sur les travaux de Romas, *Recueil des Actes de l'Académie des Sciences, Belles-Lettres et Arts de Bordeaux* **15**: 447–511, 1853. The italics in the above quotation are used by Merget (p. 484).

[107] I have searched diligently through the first three English editions and the French edition, without finding any passage such as that quoted above.

was quoting from a French review of Priestley and had never seen the original at all.

Romas tells us that on 19 October 1753 he addressed a letter to Benjamin Franklin,[108] along with two memoirs.[109] Franklin replied in a letter dated 29 July 1754, which Romas later printed in English and also in French translation, and which is reproduced in facsimile as figure 9. Although Franklin gave him "the hope that he would write again, I have never received any other letter."

On the score of Franklin's letter to him, Romas wrote:

> That which is worthy of being remarked well in this letter is that M. Franklin makes no claim to the invention of the kite [experiment]. That was, however, the time when he should have done so: he must have perceived in my letter, and more clearly still in the first memoirs [that I sent him], that I claimed to be the originator of this instrument.[110]

As a matter of fact, added Romas, he had also thought of making lightning experiments using an insulated bar (but one that ended in a ball rather than a point) in 1750, which was "more than a year before M. Franklin."[111]

Watson wrote a letter to the Abbé Nollet under the date of 15 January 1753, which was printed in French translation in a footnote to one of the two memoirs sent by Romas to Franklin, beginning: "M. Franklin has sent to the Royal Society, a fortnight ago, a very pretty electrical experiment for drawing electricity from the clouds."[112] There followed a description of the construction of the kite, precautions to be taken with it (including some not in the letter as printed in the *Philosophical Transactions*) and the experiments to be made with it. The note containing this letter then goes on to state:

> It seems by this letter that M. Franklin has used the kite prior to M. de Romas; but judging by the same letter and by the memoir of the latter, one will see that the effects were much greater at Nérac than at Philadelphia. This difference comes, it would appear, from the fact that M. de Romas garnished the cord of his kite with a metal wire, as one will see by reading his memoir.

If Franklin had flown his kite in June 1752, as I have every reason to believe he did, then Romas did not conceive of the same experiment until a month later, and since he did not fly his kite until 14 May of the following year, he thereby lost priority to Franklin both in the invention of the experiment and its performance. If Romas had actually conceived the experiment of erecting an insulated rod in 1750, it is a pity that he did not at once describe what he proposed to do and the means for doing it, since this would have given him priority in devising the first experiments to "draw the lightning from the skies." Since he neither published the idea, nor made the experiment prior to Dalibard's Marly experiment of May 1752, we are hardly entitled to give him credit for it. On the other hand, Romas may well have thought of using an insulated rod to test the electrification of clouds independently. As Franklin noted after quoting to Lining the extract from his diary about how he "came first to think of proposing the experiment of drawing down the lightning, in order to ascertain its sameness with the electric fluid," the thought "was not so much 'an out-of-the-way one,' but that it might have occurred to any electrician."[113] In any event, the possibility—even the probability— that lightning is an electrical phenonenon was not new in Franklin's day; what was original was an experiment to test this oft-expressed idea, and Franklin's was the first to be made public and to be carried into execution.

Although the note in Romas's memoir indicated that he had conceived the kite experiment independently of, but later than, Franklin, Romas—perhaps on the basis of Franklin's not asserting his own claim vigorously—concluded that Franklin might not have performed the experiment at all.

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[108] Romas (ref. 100, *supra*), 117.

[109] Romas implied that these were the two pieces published by the Académie des Sciences in the second volume of the "Mémoires des savants étrangers." If so, they were MS copies, since that volume was not published until 1755; furthermore, only one memoir by Romas appeared in the second volume, and the other one appeared in the fourth volume, but it was dated 1757!

[i] Mémoire, où après avoir donné un moyen aisé pour élever fort haut, & à peu de frais, un corps électrisable isolé, on rapporte des observations frappantes, qui prouvent que plus le corps isolé est élevé au dessus de la terre, plus le feu de l'électricité est abondant, *Mémoires de mathématique et de physique, présentés à l'Académie Royale des Sciences, par divers savans, & lûs dans les assemblées* **2**: 393–407, 1755.

[ii] Copie d'une lettre écrite à M. l'Abbé Nollet par M. de Romas [De Nérac le 26 Août 1757], *Ibid.* **4**: 514–517, 1763.

For an English account, based on [i] above, see *Gentleman's Mag.* **26**: 378–380, 1756.

[110] Romas (ref. 100, *supra*), 118.

[111] *Ibid.*, 109.

[112] Freely translated from the French; printed in Romas (ref. 109 [i], *supra*), 395n.

[113] *Benjamin Franklin's experiments* (ref. 4, *supra*), 334.

TITRES
ET PIECES JUSTIFICATIVES,
QUI prouvent que M. de ROMAS a imaginé avant M. FRANKLIN le Cerf-volant Électrique.

LETTRE DE M. FRANKLIN A M. DE ROMAS.

Philadelphia, July 29. 1754.

SIR,

YOUR moſt obliging Favour of Octob. 19 with your two very ingenious Memoirs on the ſubject of Electricity, came not to hand till yeſterday. By this Veſſel, which is juſt departing for London, I can only acknowledge the Receipt of them, and aſſure you that the Correſpondence ſo kindly offer'd willbe extreamly agreable to me. A more particular anſwer I muſt defer till the next Opportunity;

in the mean time I ſend you a late Paper of mine on Lightning, which perhaps may not be publiſhed before this reaches your Hands. I am very reſpectfully,

SIR,

Your moſt obedient humble ſerv.t
B. FRANKLIN.

M. Romas.

TRADUCTION
De la Lettre de M. Franklin à M. Romas.

De Philadelphie, le 29 *Juillet* 1754.

MONSIEUR,

LA très-obligeante lettre dont vous m'avez favoriſé le dix-neuf Octobre, & vos

FIG. 9. Franklin's letter to Romas, in reply to Romas's letter and memoirs. Reproduced from Romas, Jacques de, *Mémoire sur les moyens de se garantir de la foudre dans les maisons; suivi d'une lettre sur l'invention du cerf-volant électrique, avec les pièces justificatives de cette même lettre,* 145–146, Bordeaux, Chez Bergeret, 1776. (Reproduced from A. L. Rotch's copy, now in Houghton Library, Harvard University.)

At any rate, we know that in 1764 he asked the Academy of Sciences to adjudicate his claim to priority. Two commissioners, Nollet and Duhamel, reported as follows:

> Having regard to all these proofs, we believe that M. de Romas had not borrowed from any one the idea of applying the kite to electrical experiments, and that one must regard him as the first author of this invention, until M. Franklin or some other makes known by sufficient proofs that he had thought of it before him. (4 February 1764.)[114]

[114] Freely translated from the French; Romas (ref. 100, *supra*), 149; *cf.* M. l'Abbé Bertholon, *De l'électricité des météores* 1: 32–55, Paris, Croullebois, 1787. Also Nollet, *Lettres sur l'électricité* 2: 228–248, Paris, H. L. Guérin & L. F. Delatour, 1754, asserting Romas's claims in a letter addressed to him.

A nineteenth-century partisan of Romas, Merget, noted:

> With his ordinary prudence, Franklin . . . remained with his mouth closed, as if he recognized on his part the justice of the judgment of the Academy; but this sly resignation, did not prevent him, three years later, in 1767, from letting his friend Priestley

speak of Romas in cavalier terms which we have transcribed above.[115] One can allege, it is true, in his justification, that he was ignorant of the declaration of the commissioners of the Academy; this is very possible without being in any way probable. But that which is beyond doubt, in any case, is that he knew in their extent the pretentions of his competitors; as the latter, under the date of 19 October 1753, had sent him two memoirs [109] where these pretentions were very clearly expressed, and where the experiment of the lightning kite, recounted in all its details, is presented as an original experience.

Franklin, it is true, never entered the lists in order to defend his own claims to the prior invention of the lightning kite. In scientific matters, his procedure was always that expressed by the lawyers' phrase *res ipsa loquitur*. In his autobiography, he related that he had not personally answered any of the attacks made on his ideas by the Abbé Nollet, having "concluded to let my Papers shift for themselves; believing it was better to spend what time I could spare from public Business in making now Experiments, than in Disputing about those already made." [116] At the height of the controversy in England as to whether lightning rods should end in balls or points, he wrote to Le Roy from London (30 March 1773) that "I have an extreme Aversion to Public Altercation on Philosophic Points, and have never yet disputed with any one, who thought fit to attack my Opinions." [117] A few months later, he wrote to Ingen-Housz that he would not answer a pamphlet by Wilson "against Points . . . being averse to Disputes." [118] When Ingen-Housz was embroiled in a dispute with Priestley over the problems of photosynthesis, Franklin wrote him:

I hope you will omit the polemic piece in your French edition and take no public notice of the improper behaviour of your friend; but go on with your excellent experiments, produce facts, improve science, and do good to mankind. Reputation will follow, and the little injustices of contemporary labourers will be forgotten; my example may encourage you, or else I should not mention it. You know, that when my papers were first published, the Abbé Nollet, then high in reputation, attacked them in a book of letters. An answer was expected from me, but I made none to that book, nor to any other. They are now all neglected, and the truth seems to be established. You can always employ your time better than in polemics.[119]

"Whatever some may think and say," he wrote to Ingen-Housz, "it is worth while to do men good, for the self-satisfaction one has in the reflection." [120]

In this spirit, he undertook no dispute with Romas. And, in a later publication, he generously referred to Romas's experiments:

M. de Romas saw still greater quantities of lightning brought down by the wire of his kite. He had "explosions from it, the noise of which greatly resembled that of thunder, and were heard (from without) into the heart of the city, notwithstanding the various noises there. The fire seen at the instant of the explosion had the shape of a spindle, eight inches long and five lines in diameter. Yet, from the time of the explosion to the end of the experiment, no lightning was seen above, nor any thunder heard. At another time the streams of fire issuing from it were observed to be an inch thick and ten feet long." [121]

Priestley devoted considerable space to Romas in his history and noted:

The greatest quantity of electricity that was ever brought from the clouds, by any apparatus prepared for that purpose, was by Mr. De Romas, assessor to the presideal of Nerac. This gentleman was the first who made use of a wire interwoven in the hempen cord of an electrical kite. . . .[122]

Franklin's friend and co-experimenter Kinnersley also performed kite experiments. They are described in a letter written to Franklin on 12 March 1761, as follows:

Whether the electricity in the air, in clear, dry weather, be of the same density at the height of two or three hundred yeards, as near the surface of the earth, may be satisfactorily determined by your old experiment of the kite. The twine should have throughout a very small wire in it, and the ends of the wire, where the several lengths are united, ought to be tied down with a waxed thread, to prevent their acting in the manner of points. I have tried

[115] Merget (ref. 106, *supra*), 490–491. Merget had reference to the following statement "from Priestley": "MM. de Lor et d'Alibard, dit-il, firent également l'expérience du cerf-volant en Angleterre, l'année suivante (ce qui est complètement faux), et M. de Romas voulant s'assurer par lui-même de ce qu'il entendait raconter à ce sujet, la répéta en France avec beaucoup plus d'appareil." My comments in footnote 107, *supra*, apply equally well here.

[116] Farrand (ref. 15, *supra*), 384.

[117] Smyth (ref. 33, *supra*) **6**: 28–29.

[118] *Ibid.*, 141–143.

[119] *Ibid.* **1**: 14.

[120] *Idem.*

[121] *Ibid.* **5**: 422n.

[122] Priestley (ref. 8, *supra*) **1**: 411.

the experiment twice, when the air was as dry as we ever have it, and so clear that not a cloud could be seen, and found the twine each time in a small degree electrized positively. The kite had three metalline points fixed to it; one on the top, and one on each side. That the twine was electrized, appeared by the separating of two small cork balls, suspended on the twine by fine flaxen threads, just above where the silk was tied to it, and sheltered from the wind. That the twine was electrized positively, was proved by applying to it the wire of a charged bottle, which caused the balls to separate further, without first coming nearer together. This experiment showed, that the electricity in the air, at those times, was denser above than below. But that cannot be always the case; for, you know, we have frequently found the thunder-clouds in the negative state, attracting electricity from the earth; which state, it is probable, they are always in when first formed, and till they have received a sufficient supply. How they come afterwards, towards the latter end of the gust, to be in the positive state, which is sometimes the case, is a subject for further inquiry.[123]

Nor was Kinnersley the only American to repeat Franklin's experiment; another was John Lining. In a letter dated 14 January 1754, Lining described his kite in the following terms:

> The kite, which I used, was made in the common way; only, in place of paper, I covered it with a silk, called *alamode*. The line was a common small hempen one of three strands. A silk line, except it had been kept continually wet, would not conduct the electricity; and a wire, besides other inconveniences, would have been too heavy. I had not any instrument, whereby I could take the height of the kite; but, I believe, it was at least 250 feet high. It was flown in the day-time.[124]

Lining evidently used a key at the end of the kite-string, just as Franklin had, and he charged a Leyden jar from it, repeating the experiments usually made with the electrical machine at its prime conductor.

Another mid-eighteenth-century physicist to experiment on atmospheric electricity with a kite was the Abbé Beccaria, a staunch Franklinist, who did much to promote the use of lightning rods in Italy and who appears to have been the first person to be successful at electrolyzing metallic compounds. In one of his writings on atmospheric electricity, he related:

> It was in the year 1756, that the frequent and continued use of kites, which other observers only used to make researches on the electricity of clouds, procured me a confirmation of what I had till then only conjectured, that is to say, that even during clear weather (except in the cases of a great dampness of the air, or of an impetuous wind) a mild weak electricity perpetually took place.
>
> Kites were most useful instruments to me, for such first experiments on the state of the atmosphere. They rise to a great height, to a region where the difference of the atmospheric electricity uses to be greater; they gather great quantities of this electricity, by means of the pack-thread which holds them, and they retain it the better as they are capable of being insulated. . . . Now, a string made of the best silk, of a small diameter, and of great length insulates a kite extremely well; and it is an easy matter to keep it dry by warming it, or to change it, when it grows damp. Though I was at first ignorant of the contrivance of Sig. Romas, who interweaves the string which holds his kite with thin metallic wires, the same thought occurred to me the more naturally, as I was then exploring the accidents of the weaker electricity that takes place in serene weather.[125]

Accordingly to Priestley,

> He made use both of kites and pointed rods, and of a great variety of both at the same time, and in different places. Some of the strings of his kites had wires in them, and others had none. Some of them flew to a prodigious height, and others but low; and he had a great number of assistants, to note the nature, time, and degree of appearances, according as his views required.
>
> To keep his kites constantly insulated, and at the same time to give them more or less string, and for many other purposes, he had the string rolled upon a reel, which was supported by pillars of glass; and his conductor had a communication with the axis of the reel.[126]

Yet another to make experiments on atmospheric electricity with a kite was Peter Van Musschenbroek, one of the discoverers of the Leyden jar or condenser.[127] It should be noted, however, that

[123] Sparks (ref. 7, *supra*) **5**: 370–371.

[124] *Phil. Trans. Roy. Soc.* **48** (2): 758, 1754. *Cf. Gentleman's Mag.* **23**: 431, 1753.

[125] Beccaria, Giambatista, *A treatise upon artificial electricity . . . to which is added an essay on the mild and slow electricity which prevails in the atmosphere during serene weather, translated from the original Italian*, 449–450, London, J. Nourse, 1776. For Franklin's high opinion of Beccaria's work, see his letter to Dalibard (ref. 44, *supra*).

[126] Priestley (ref. 8, *supra*) **1**: 396.

[127] *Cf.* Mottelay, Paul Fleury, *Bibliographical history of electricity and magnetism*, 320, London, Charles Griffin, 1922, for a table of eighteenth-century experiments on atmospheric electricity.

apparently none of these experiments was made during a severe lightning storm, that in all cases the key and the kite string or wire were charged by electrostatic induction, and that the kite was used to advance knowledge of the electrification of the atmosphere even in serene weather. Thus the kite provides us with an example of the way in which a tool invented to solve a specific problem (the possible electrification of clouds) finds application in investigating a much larger class of phenomena.

9. CONCLUSION

Since electrical kites were flown by others, there is no reason to suppose that one might not have been flown by Franklin. Priestley's testimony, approved by Franklin, set the date of the experiment in June 1752, one month after the performance of Franklin's earlier (sentry-box) experiment. The implication of Franklin's letter of 1 October 1752, describing the kite to Collinson, is that some sort of metal "points" had been erected in Philadelphia in June 1752, hence earlier than the rod (with the warning bells) erected by Franklin on his own house in Philadelphia in September 1752.

No information has as yet been uncovered, despite a considerable research by a number of different individuals, to make us reject the testimony of Franklin and Priestley. Although a number of perplexing questions have been raised, they may all be answered in a reasonable way, although some conjecture is required on occasion, e.g., to explain the delay in publishing the experimental results. Further confidence in the accounts by Franklin and Priestley arises when we review the accumulated collateral material on lightning and lightning rods in the period from June to October 1752, all of which is consistent with the dates and other information they provided. In any event, it is difficult to think that Franklin, who was always exceedingly honest in reporting scientific information, would have in any way falsified the record.

I do not believe that all relevant information about this episode has been uncovered. Everyone who has ever investigated the question has been able to add something new. Perhaps the discovery of hitherto unknown Franklin correspondence or of diaries and letters of his friends or fellow inhabitants of Philadelphia, will some day reveal more details about the kite itself and the Philadelphia lightning rods. But, as matters stand now, in June 1952, two hundred years after the time when Priestley asserted with Franklin's approval that the kite was flown, I believe we are justified in celebrating the two-hundredth anniversary of the lightning kite experiment and the two hundredth anniversary of the first lightning rods—erected in Philadelphia in June or July 1752 soon after the kite was flown.[128]

[128] I am fully aware that many more statements in this article should contain such words as "very likely," "possibly," "probably," "may very well have," etc. Yet the reader will not, I am sure, have had any difficulty in separating statements of fact from interpretation. It is plain that I have followed a policy of believing what Franklin wrote or told Priestley and then seeing whether every other available scrap of information can be given a plausible explanation that squares with the Franklin-Priestley chronology. The critical reader must decide for himself whether this has been successful, keeping in mind that a different fundamental premise—e.g., that Franklin flew his kite only after learning about the Marly experiments—would lead to wholly different interpretations. I hope that some readers will be tempted to challenge some of my interpretations and conclusions and uncover further information in the process, so that I hope—to quote Franklin—that this article may excite "the attention of the ingenious to the subject, and so become the occasion of more exact disquisition and more compleat discoveries."